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MIND

A QUARTERLY REVIEW

OF

PSYCHOLOGY AND PHILOSOPHY

EDITED BY

Prof. GILBERT RYLE

WITH THE CO-OPERATION OF PROF. SIR F. C. BARTLETT AND PROF. C. D. BROAD

CONTENTS

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By IRVING M. COPI

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1. *The " Tractatus " and ordinary language*

In Mrs. Daitz's criticism of Wittgenstein's "assimilation of sentences to facts" (p. 188) she writes that "This view brings with it the consequence that all ordinary sentences have, for fact-stating purposes, one word too many!" (p. 189). This criticism is damaging if the *Tractatus* is intended as an account of ordinary sentences, for not all of its assertions are true of ordinary sentences—as Mrs. Daitz correctly maintains. On the other hand, if the *Tractatus* is *not* intended as an account of ordinary sentences, then it is no objection that its assertions are not true of ordinary sentences. One might with equal justice condemn a Latin grammar because its assertions are not true of Chinese sentences.

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1. *The " Tractatus " and ordinary language*

In Mrs. Daitz's criticism of Wittgenstein's " assimilation of sentences to facts " (p. 188) she writes that " This view brings with it the consequence that all ordinary sentences have, for fact-stating purposes, one word too many ! " (p. 189). This criticism is damaging if the *Tractatus* is intended as an account of ordinary sentences, for not all of its assertions are true of ordinary sentences—as Mrs. Daitz correctly maintains. On the other hand, if the *Tractatus* is *not* intended as an account of ordinary sentences, then it is no objection that its assertions are not true of ordinary sentences. One might with equal justice condemn a Latin grammar because its assertions are not true of Chinese sentences.

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Although scores of his propositions contain special symbols and are concerned with special symbolisms, Wittgenstein certainly says *some* things about ordinary sentences. For example, he remarks that "Colloquial language is a part of the human organism and is not less complicated than it ", and goes on to say that "The [tacit conventions] [1] to understand colloquial language are enormously complicated " (4.002). He admits that the picture theory of meaning does not seem to be immediately applicable to ordinary language, "At the first glance the proposition—say as it stands printed on paper—does not seem to be a picture of the reality of which it treats . . . " (4.011).

Wittgenstein does not seem to maintain a completely consistent attitude towards ordinary language. Apparent approval is suggested by "All propositions of our colloquial language are actually, just as they are, logically completely in order " (5.5563). But the reverse is expressed by "In the language of everyday life it very often happens that the same word signifies in two different ways. . . . Thus there easily arise the most fundamental confusions (of which the whole of philosophy is full) " (3.323, 3.324). Ordinary language is said to be obscure and misleading : "That the propositional sign is a fact is concealed by the ordinary form of expression, written or printed . . . " (3.143). "From it [colloquial language] it is humanly impossible to gather immediately the logic of language. . . . Language disguises the thought " (4.002).

The tendency to reject ordinary language seems to me to predominate. Wittgenstein was concerned with the construction of "an adequate notation " (6.122), writing : "In order to avoid these errors [the most fundamental confusions (of which the whole of philosophy is full)], we must employ a symbolism which excludes them. . . . A symbolism, that is to say, which obeys the rules of *logical* grammar—of logical syntax " (3.325). Symbolic logic was hailed as a start in the right direction : "The logical symbolism of Frege and Russell is such a language, which, however, does still not exclude all errors " (3.325). The *Tractatus* contains several definite proposals for improving that symbolism by altering it in the direction of greater artificiality. For example "Identity of the object I express by identity of the sign and not by means of a sign of identity . . . " (5.53). "I write therefore not ' f(a,b) . a = b ', but ' f(a,a) ' . . . " (5.531). Wittgenstein concluded that : "The identity sign is therefore not an

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that apparent propositions like: " $a = a$ ", . . ., cannot be
written in a correct logical notation at all " (5.534).

I understand Wittgenstein to be primarily concerned with
specifications for an artificial symbolic language which would
conform to " the rules of logical grammar " and thereby exclude
" fundamental confusions (of which the whole of philosophy is
full) ". Or, as Russell says in his Introduction, " . . . he is
concerned with the conditions which would have to be fulfilled by
a logically perfect language " (p. 7). If this interpretation is
correct, then to point out that Wittgenstein's assertions are not
true of ordinary sentences is no criticism of his doctrines. And to
point out that his assertions are true of this or that ordinary
sentence is altogether beside the point. For this reason Mrs.
Daitz's and Mr. Evans's extended discussion of the sentence
" Sophia hates Amos " seems to me wholly irrelevant to any
appraisal of Wittgenstein's doctrines.

In the following sections it will be instructive to set forth some
of the reasons why such a sentence as " Sophia hates Amos "
cannot be an elementary proposition in the sense of the *Tractatus*.

2. *Wittgenstein's restricted picture theory of meaning*

Wittgenstein expounds a " picture theory of meaning " for
propositions, writing : " The proposition is a picture of reality . . ."
(4.01, 4.021). " The proposition only asserts something, in so far
as it is a picture " (4.03). " Propositions can be true or false only
by being pictures of the reality " (4.06).

The pictorial nature of propositions was not simply *assumed*.
It was *inferred* by Wittgenstein from what he regarded as an
essential characteristic of propositions, that a person who knows
the language can understand a proposition he has never seen or
heard before. " It is essential to propositions, that they can
communicate a *new* sense to us " (4.027). The inference was
given three different formulations. First : " In order to under-
stand the essence of the proposition, consider hieroglyphic
writing, which pictures the facts it describes. And from it came
the alphabet without the essence of the representation being lost.
This we see from the fact that we understand the sense of the
propositional sign, without having had it explained to us "
(4.016, 4.02). Second : " The proposition is a picture of reality,
for I know the state of affairs presented by it, if I understand the
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The proposition communicates to us a state of affairs, therefore it must be *essentially* connected with the state of affairs. And the connection is, in fact, that it is its logical picture " (4.03).

These various formulations of Wittgenstein's argument clarify the intended scope of his picture theory of meaning. His conclusion asserts the picture theory of meaning for composite signs only, that is, for propositions or propositional signs, but not for simple signs or words.

Nowhere does Wittgenstein assert or suggest that simple signs, words, or names are pictures. By implication, the picture theory is denied for non-composite signs when it is said that pictures must be composite : " The picture consists in the fact that its elements are combined with one another in a definite way " (2.14). The one argument intended to establish the picture theory for propositions cannot possibly be applied to words, for its premiss attributes to propositions a characteristic that is explicitly denied to single words. Propositional signs are said to be pictures because ". . . we understand the sense of the propositional sign, without having had it explained to us " (4.02). But " The meanings of the simple signs (the words) must be explained to us, if we are to understand them " (4.026).

For these reasons I think it incorrect to interpret the *Tractatus* as presenting a picture theory of words. Even apart from the foregoing considerations, it is surprising that Mrs. Daitz should assert of " the ' picture ' elucidation of names " that it " may be put as ' The name means the object, the object is its meaning ' " (p. 198). Wittgenstein's proposition 3.203 (" The name means the object. The object is its meaning. ") clearly formulates a *denotation* theory of meaning for names. It asserts that for a name to have a meaning there must exist an object that it names. But it has nothing to do with *pictures*. In fact, since a picture has a meaning even if there exists no object that it pictures, a picture theory of meaning for names is incompatible with a denotation theory of meaning for them. Thus Wittgenstein's proposition 3.203 implies the denial of the picture theory of meaning for names, and is not a " way of putting " that theory. Therefore even had Mrs. Daitz succeeded in showing that ". . . the Picture Theory . . . *must* be a misdescription of ' how words mean ' " (p. 184), she would not thereby have damaged the *Tractatus*. For there Wittgenstein does not assert that the picture theory describes ' how words mean ', but in effect denies it.

Wittgenstein's picture theory of meaning is even further restricted. Intended for propositions only, it applies not to *all* propositions, but to elementary propositions alone. Different

passages suggest more or less clearly that elementary propositions only are pictures. The implicit arguments vary in the degree of their cogency, but it seems worthwhile to indicate at least three of them.

First, in explaining how propositions can have sense Wittgenstein is led to assert that *what* they picture or present are atomic facts : " One name stands for one thing, and another for another thing, and they are connected together. And so the whole, like a [tableau vivant], presents the atomic fact " (4.0311). In a later passage he remarks that : " The simplest proposition, the elementary proposition, asserts the existence of an atomic fact " (4.21). Granted that propositions which are pictures assert atomic facts, and that propositions which assert atomic facts are elementary propositions, it follows that propositions which are pictures are elementary propositions.

A second argument involves as premiss : " The elements of the picture stand, in the picture, for the objects " (2.131). Now all the elements of an elementary proposition stand for objects, because " The elementary proposition consists of names " (4.22) and " In the proposition the name [stands for] the object " (3.22). But non-elementary propositions are " . . . truth-functions of elementary propositions " (5, see also 5.3, 5.54, 6.001). And " All truth-functions are results of the successive application of a finite number of truth-operations to elementary propositions " (5.32, see also 5.234, 5.5). Hence non-elementary propositions contain truth-operations or logical constants. But those truth operations or logical constants do not stand for objects : " My fundamental thought is that the ' logical constants ' do not [stand for objects] " (4.0312). " And if there were an object called ' $\sim$ ', then ' $\sim\sim p$ ' would have to say something other than ' p '. For the one proposition would then treat of $\sim$, the other would not " (5.44). In short, non-elementary propositions contain elements which do not stand for objects, but all elements of pictures must stand for objects, therefore non-elementary propositions cannot be pictures. Hence Wittgenstein intended his picture theory of meaning for elementary propositions only.

A third argument concerns the numbers of elements in pictures and in what they picture. Mrs. Daitz correctly reports that " Wittgenstein asserts a correspondence between elements of the sign and elements of the signified . . . " (p. 186). But it is not true that he " . . . does not go on to assert that this correspondence is one to one " (p. 181). The one to one nature of the correspondence is definitely asserted : " In the proposition there must be exactly as many things distinguishable as there are in the

state of affairs, which it represents. They must both possess the
same logical (mathematical) multiplicity . . . " (4.04). That
pictures and states of affairs pictured have the same multiplicity
follows from Wittgenstein's earlier remarks that " To the objects
correspond in the picture the elements of the picture " (2.13) and
" The elements of the picture stand, in the picture, for the objects"
(2.131). An elementary proposition and the state of affairs it
represents have the same multiplicity, hence the elementary
proposition can be a picture : " One name stands for one thing,
and another for another thing, and they are connected together.
And so the whole, like a [tableau vivant], presents the atomic
fact " (4.0311). The preceding remark holds for elementary
propositions, since " The simplest proposition, the elementary
proposition, asserts the existence of an atomic fact " (4.21). How-
ever, the alleged one to one correspondence obviously fails to hold
for non-elementary propositions. To any proposition ' p ' there
corresponds the logically equivalent non-elementary proposition
' $\sim\sim p$ '. Identically the same fact is asserted both by ' p ' and
by ' $\sim\sim p$ ' for as Wittgenstein argues : " That from a fact p an
infinite number of others should follow, namely $\sim\sim p, \sim\sim\sim\sim p$,
etc., is indeed hardly to be believed " (5.43). Since ' $\sim\sim p$ '
contains more elements than ' p ', they do not have the same
multiplicity. Hence if non-elementary as well as elementary
propositions had the same multiplicity as the facts they assert,
there would follow the absurd conclusion that one and the same
fact must contain different numbers of objects. The present
argument is in effect a *reductio ad absurdum* proof that the picture
theory of meaning is intended for elementary propositions only.

When understood in this restricted sense, the picture theory of
meaning is not damaged by the usual ' refutations ' of its critics.
For example, Mrs. Daitz argues persuasively that " . . . negative
statements, conditional statements, disjunctive statements, etc.,
[are] not describable in picture terms," and that they are
" . . . statements which have no pictorial counterparts " (p. 196).
But these are non-elementary propositions, to which Wittgen-
stein's picture theory was not meant to apply.

Mrs. Daitz bases another ' refutation ' of the picture theory of
meaning on an alleged fundamental difference between pictures
and languages : " . . . in the sense in which there are foreign
languages there are no foreign pictures " (p. 199). The implica-
tions of this alleged fundamental difference are clear : " . . . we
comprehend a picture on seeing it for the first time. No explana-
tion is necessary . . . ; the picture ' speaks ' for itself, it shows
what it pictures . . . " (p. 199). But " A sentence does not show

its meaning—consider the impenetrability of a sentence in an un-mastered language. To understand a statement we must first have learnt the language in which it is made . . . " (p. 199).

Mrs. Daitz believes these implications to refute the doctrines of the *Tractatus* : " Only if learning was unnecessary for languages too would Wittgenstein be right in saying ' 4.03 A proposition must communicate a new sense with old words. The proposition communicates to us a state of affairs, therefore it must be es-sentially connected with the state of affairs.' We do not understand a sentence on first hearing it because it is *essentially* connected with reality ; a sentence is *conventionally* connected with reality " (p. 199).

Before examining this ' refutation ', two remarks are in order. First, Wittgenstein knew very well that foreign languages must be learned before propositions expressed in them can be understood. When he wrote " I understand the proposition, without its sense having been explained to me " (4.021), he was clearly presupposing a mastery of the language. For of the proposition (sentence, statement) he wrote " One understands it if one understands its constituent parts " (4.024), and he insisted that the meanings of those constituent parts must be *learned* : " The meaning of the simple signs (the words) must be explained to us, if we are to understand them " (4.026). Hence Wittgenstein does not deny " the impenetrability of a sentence in an unmastered language ".

And second, Wittgenstein does not deny that " a sentence is *conventionally* connected with reality ", though he would insist that that is not the whole story. " In our notations there is in-deed something arbitrary, but *this* is not arbitrary, namely that *if* we have determined anything arbitrarily, then something else *must* be the case. (This results from the *essence* of the notation) " (3.342). Wittgenstein explicitly denies that the connection between propositions and reality must be either wholly essential or wholly accidental (conventional), writing " A proposition possesses essential and accidental features . . . " (3.34). The connection between words and objects is conventional or arbitrary, and must be learned. But the connection between the structure of a proposition and the structure of the fact asserted by it is not wholly conventional. One essential connection, their having the same logical (mathematical) multiplicity, has already been mentioned and will be discussed further in the following section.

Mrs. Daitz's criticism involves the following argument : Since there are foreign languages but no foreign pictures, understanding a picture requires no previous learning, whereas understanding a proposition does require previous learning ; hence no proposition

is a picture. There are two presuppositions underlying this objection. The first can scarcely be challenged : there are foreign languages, so understanding a proposition requires previous learning.

The second presupposition is that there are no foreign pictures, so understanding a picture requires no previous learning. In sharp contrast to the first, this second presupposition is very questionable. *Are* there no foreign pictures ? *Do* we comprehend *any* picture on seeing it for the first time ? Is no explanation of any picture ever necessary ? Does the picture ' speak ' for itself to every beholder, regardless of his ignorance in matters of style and idiom ? Just to ask these questions is to expose the error of Mrs. Daitz's second presupposition.

There may be *some* pictures whose content or subject matter is immediately evident to anyone who looks at them. But many pictures require much learning for their interpretation. These include maps, diagrams, political cartoons, cubist paintings, and pictures in religious symbolism ; all unintelligible to the uninitiated. Even perspective had to be learned. It seems, therefore, that the alleged fundamental difference between pictures and propositions involves a fundamental mistake. It is simply false that " . . . in the sense in which there are foreign languages there are no foreign pictures " (p. 199).

The present objection to the picture theory of meaning rests on two presuppositions. The first is admitted to be a truism, but the second is patently false. These remarks are not intended to demonstrate the truth of the picture theory of meaning. They are intended to show only that the usual refutations of it are not convincing.

3. *Relations*

Around the beginning of the present century many philosophers were concerned with problems involving relations. In his *Principles of Mathematics* Russell carefully examined Bradley's well known argument against relations, and concluded that " the endless regress, though undeniable, is logically quite harmless " (p. 100). Bradley is nowhere mentioned in the *Tractatus*, nor are his views referred to explicitly, but Wittgenstein there develops a theory of relations that permits another and quite different reply to Bradley's argument. That reply is intimately connected with his picture theory of meaning, which must now be examined in greater detail.

Wittgenstein declares an atomic fact to be a " combination " or " configuration " of objects (2.01, 2.0272). A picture is a fact

(2.141) ; " The picture consists in the fact that its elements are combined with one another in a definite way " (2.14). The mode of combination of elements of a picture represents the mode of combination of objects in the atomic fact that is pictured by it : " That the elements of the picture are combined with one another in a definite way, represents that the things are so combined with one another " (2.15). Similarly, an elementary proposition is " a connexion, a concatenation, of names " (4.22). It too is a fact : " The propositional sign consists in the fact that its element, the words, are combined in it in a definite way. The propositional sign is a fact " (3.14). It is the mode of union of its constituent elements that makes it a picture and determines the *sense* of a proposition : " The proposition is a picture of its state of affairs, only insofar as it is logically articulated " (4.032). " One name stands for one thing, and another for another thing, and they are connected together. And so the whole, like a [tableau vivant], presents the atomic fact " (4.0311, see also 3.1431). As in the case of pictures and their elements, the mode of combination of names in a propositional sign represents the mode of combination of objects in the atomic fact that is asserted by it : " To the configuration of the simple signs in the propositional sign corresponds the configuration of the objects in the state of affairs " (3.21). In brief, an atomic fact, which consists of objects related to each other, is asserted by an elementary proposition, which consists of names related to each other. Each name in the proposition represents an object in the fact, and the relation of the names represents the relation of the objects.

Against this view Mrs. Daitz objected that

> . . . converting sentences into facts creates a difficulty : in conversion, an n-termed sentence becomes an n + 1-termed fact. Consider ' Sophia hates Amos '. This becomes the fact ' *Hates* ' *is between* ' *Sophia* ' *and* ' *Amos* ', which has the consequences that it is impossible to gear the form of a sentence to form of the fact. The sentence ' Sophia hates Amos ' is not identical in form with, *i.e.* has not the same number of elements as, the fact *Sophia hates Amos*. For the sentence is the fact ' *Hates* ' *is between* ' *Sophia* ' *and* ' *Amos* ', *i.e.* it has four elements while *Sophia hates Amos* has only three. This view brings with it the consequence that all ordinary sentences have, for fact-stating purposes, one word too many ! (p. 189, see also p. 196).

In his defence of Wittgenstein from this criticism Mr. Evans agrees with Mrs. Daitz on the number of elements in the sentence, writing : " Now the four elements into which Wittgenstein converts

' *aRb* ' are evidently the three signs and their order " (p. 259).
But he nevertheless defends the view that a sentence and the fact
asserted by it have the same multiplicity, arguing that the fact as
well as the sentence contains an extra element.   Mrs. Daitz asser-
ted that the fact *Sophia hates Amos* contains only three elements.
But Mr. Evans counts it as containing four elements, writing :
" Wittgenstein would have said, I think, that the fact that Sophia
hates Amos contained four elements : the two people, the hating,
and the structure of these . . . " (p. 260).

Let us discuss this disagreement in terms of the fact *that aRb*
rather than the fact *that Sophia hates Amos*, for the latter involves
difficulties irrelevant to the present issue.   Both Mrs. Daitz and
Mr. Evans count four elements in the fact ' *aRb* ', but they
disagree on the number of elements in the fact *that aRb*.   In
the latter fact Mrs. Daitz can count only three elements whereas
Mr. Evans can count four.

The four elements counted by Mr. Evans in the fact *that aRb*
are the two objects *a* and *b*, the relation *R*, and the structure of
these.   The fourth element cannot be the fact itself *that aRb*,
since *elements of* that fact are being enumerated ; it must be taken
to mean a relation whose relata are the first three elements
enumerated : *a, b*, and *R*.[1]   But if *R* is not sufficiently related to *a*
and *b* by relating *them*, so that a new (three-termed) relation is
required, then that three-termed relation is not sufficiently
related to *a* and *b* and *R* by relating *them*, and a new (four-termed)
relation is required, which would constitute a *fifth* element in the
now fast growing fact *that aRb*.   Clearly we are well into Bradley's
regress.   On the other hand, there may be some good reason for
taking exactly one step into Bradley's regress.   If so, there
would be exactly four elements in the fact.   But Mr. Evans has
neither presented any such reason nor given any hint as to what
it might be.

Mrs. Daitz and Mr. Evans agree that there are four elements in
the elementary proposition ' *aRb* '.   To find four elements in a
sequence of three words or symbols one must count more than just
words as elements.   Mr. Evans states explicitly that each sign is
an element and that their order is an additional element (p. 259).
Mrs. Daitz evidently shares this view, since she finds the same num-
ber of elements.   But Wittgenstein's conception of a propositional
element is quite different ; " The propositional sign consists in
the fact that its elements, the words, are combined in it in a
definite way . . ." (3.14).   Also, " These elements [of the
propositional sign] I call ' simple signs ' . . . " (3.201).   " The
simple signs employed in propositions are called names " (3.202).

For Wittgenstein the elements of an elementary proposition are the words (names or simple signs) occurring in it ; their order or arrangement is not regarded by him as an additional element. Hence for Wittgenstein the elementary proposition ' *aRb* ' contains *three* elements, not four.

It might be thought that the conclusion reached in the preceding paragraph would automatically settle the issue between Mrs. Daitz and Mr. Evans as to the number of elements in the fact *that aRb*. Atomic fact and elementary proposition " must both possess the same logical (mathematical) multiplicity " (4.04) and the elementary proposition ' *aRb* ' contains three elements, whence the atomic fact *that aRb* must also contain three elements. The issue, however, is not so easily resolved. Like the picture theory of meaning, the ' multiplicity ' doctrine does not apply to ordinary language. Although the ordinary language proposition " ambulo " contains but a single word, it is nevertheless composite (4.032). In ordinary language, therefore, the number of elements contained in a proposition does not determine that proposition's multiplicity, hence the multiplicity of an ordinary sentence is not well defined. The ' multiplicity doctrine ' (of 4.04) applies not to ordinary language but only to " a symbolism . . . which obeys the rules of *logical* grammar—of logical syntax ". Before we can use the multiplicity doctrine to determine the number of elements in the atomic fact *that aRb* we must first discover how to express it in " logical symbolism " (3.325), " logical notation " (5.533), " correct logical notation " (5.534), or " an adequate notation " (6.122).

The key to this problem is found in 3.1432

> We must not say, " The complex sign ' *aRb* ' says ' *a* stands in relation *R* to *b* ' " ; but we must say, " *That* ' *a* ' stands in a certain relation to ' *b* ' says *that aRb* ".

The negative injunction here expressed should not be taken simply to forbid use of the locution " The complex sign ' *aRb* ' . . ." *in* " an adequate notation ", for that prohibition is enjoyed elsewhere. A propositional sign, being a fact, cannot be named (3.144, see also 3.24, 4.023). Hence putting quotation marks around a sentence does not form its name. Apart from truth operators, propositions contain only names (3.202, 4.22, 4.221, 5.55). Hence in no " symbolism . . . which obeys the rules of *logical* grammar " can we say " The complex sign ' *aRb* '. . . ".

Paragraph 3.1432 should rather be taken to forbid using the locution " The complex sign ' *aRb* ' says ' *a* stands in relation *R* to *b* ' " *of* " an adequate notation ". In ordinary language and also in the not yet adequate notation of Frege and Russell (3.325) the

fact *that aRb* is expressed in a sentence ' *aRb* ' containing the
three words ' *a* ', ' *b* ', and ' *R* '. But not in the " adequate nota-
tion " Wittgenstein recommends. If it were, he would have
written " . . . we must say, ' *That* " *a* " stands in a certain rela-
tion to " *b* " and to " *R* " says *that aRb* '." But he did not. He
wrote instead " . . . we must say, ' *That* " *a* " stands in a certain
relation to " *b* " says *that aRb* '." The difference is crucial. In
" an adequate notation " no propositional sign asserting *that aRb*
contains the relation symbol ' *R* '. Names for the relata *a* and *b*
appear, but no name for the relation *R*. Here it must be remem-
bered that the language *used* is not the " adequate notation "
mentioned. Thus Wittgenstein can *use* ' *aRb* ' to say *that aRb*
while insisting that it not be done (in " an adequate notation ").

The fact *that aRb* is expressed in " an adequate notation " by
a proposition containing only the two words ' *a* ' and ' *b* '. Since
only words count as elements, the proposition contains exactly
two elements, and by the multiplicity doctrine, the fact *that aRb*
contains exactly two elements. Those two elements are the
objects *a* and *b* named by the proposition's elements ' *a* ' and ' *b* '
(3.203).

Since Wittgenstein conceives the fact *that aRb* to contain just
two elements, Mrs. Daitz's contention that there are three is
closer then Mr. Evans' contention that there are four, but neither
is in agreement with Wittgenstein's count. Both Mrs. Daitz and
Mr. Evans count four elements in an ordinary sentence such as
' *aRb* '. Since Wittgenstein counts only words as elements, he
would count only three elements in that sentence. Here too
Mrs. Daitz and Mr. Evans are in disagreement with Wittgenstein.

Nevertheless, since the fact *that aRb* contains two elements and
the ordinary sentence ' *aRb* ' contains three elements, Mrs. Daitz
is correct in her conclusion that " ordinary sentences have, for fact-
stating purposes, one word too many " (p. 189). The " one word
too many " in ordinary sentences is in every case a relation word.
But the extraordinary sentences of the " logical symbolism "
recommended by Wittgenstein contain no relation words. Hence
they do not contain " too many " words, and Mrs. Daitz's
criticism falls wide of the mark. We have here, incidently, one
reason why the sentence " Sophia hates Amos " cannot be an
elementary proposition in an " adequate notation ". That sen-
tence contains the relation word " hates ", whereas no relation
word can occur in a " logical notation ".

The question naturally arises : if propositional signs contain no
words for relations, how can they express relational facts ?
Wittgenstein proposes to express a relation of objects by a relation

of their names : " *That* ' *a* ' stands in a certain relation to ' *b* ' says *that aRb* " (3.1432, see also 3.1431). Against this proposal Mrs. Daitz argues that relation words are necessary, and cannot be eliminated in favour of relations *of* words :

> A sentence needs a mark where a picture does not : it cannot show that this adjoins that, but must *say* so. . . . Say we drop the word for *adjoins* and write ' This that ' in place of ' This adjoins that '. How would we indicate that this adjoins as opposed to surpasses or divides into that ? (p. 196).

Mrs. Daitz's present objection had been anticipated by Russell in *An Outline of Philosophy* :

> Take, say, the fact that lightning precedes thunder. If we were to express this by a language closely reproducing the structure of the fact ; we should have to say simply : " lightning, thunder ", where the fact that the first word precedes the second means that what the first word means precedes what the second word means. But even if we adopted this method for temporal order, we should still need words for all other relations, because we could not without intolerable ambiguity symbolize them also by the order of our words (pp. 60, 275).

This objection presupposes that only one relation can significantly obtain between signs. (It is immaterial whether it is Mrs. Daitz's *adjoining* or Russell's *preceding*.) Mrs. Daitz seems to have considered just one kind of case : " A drawing of one thing adjoining another needs a mark for one thing and a mark for the other, but no mark for the relation of adjoining. This it shows by the spatial placing of the marks for the two things " (p. 196).

In the case of adjacency, the pictured relation is represented by the same relation of parts of the picture. But not all relations among pictured objects are represented by placing the pictorial representations of those objects in identically the same relation that is to be represented. In painting, the relation *more distant than* is sometimes represented by the relation *bluer than*, sometimes by the relations *higher than* or *smaller than*. Although in pictorial representation relations are represented by relations, they are often represented by relations other than themselves.

Once it is understood that the picturing relation need not be the same as the relation pictured, it is easy to see how the picture theory of meaning can apply to relational propositions in general. Any relation of objects, spatial or non-spatial, can be represented by a spatial relation of the names of those objects. That *a* has relation *R* to *b* can be represented by writing ' *a* ' some specified

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distance and direction from ' b ', and that a has some different relation R' to b can be presented by writing ' a ' some different distance and direction from ' b '. Wittgenstein described this method of representing relations explicitly : " The essential nature of the propositional sign becomes very clear when we imagine it made up of spatial objects (such as tables, chairs, books) instead of written signs. The mutual spatial position of these things then expresses the sense of the proposition " (3.1431).

A plurality of significant relations among signs is evident in elementary mathematics, where we use different spatial relations of the symbols for x and y to express their product, x raised to the yth power, y raised to the xth power, the xth y in a sequence of y's, and the yth x in a sequence of x's (xy, x^y, y^x, y_x, x_y). Since a relation can be represented by a relation other than itself, indefinitely many relations among objects can be represented by the indefinitely many spatial relations among the names of those objects, without any ambiguity in the symbolization. Once the error of its presupposition is revealed, the Russell-Daitz objection collapses, and Wittgenstein's proposal to eliminate relation words in favour of relations is seen to be entirely feasible.

To see how the *Tractatus* provides an answer to Bradley's argument against relations, it will be helpful to review Russell's discussion of the problem. His *Principles of Mathematics* states " . . . that the endless regress, though undeniable, is logically quite harmless " (p. 100). Later, however, Russell seemed no longer to be satisfied that the ' undeniable ' regress was harmless, for he began to insist that the regress was deniable, or perhaps better, avoidable. His diagnosis of the original difficulty appears in *An Outline of Philosophy* :

> A great deal of the confusion about relations which has prevailed in practically all philosophies comes from the fact that relations are indicated, not by relations, but by words which are as substantial as other words (p. 275).

Bradley's argument in particular was said to be grounded in that linguistic fact :

> Bradley conceives a relation as something just as substantial as its terms, and not radically different in kind. . . . I think Bradley has been misled, unconsciously, by . . . the fact that the *word* for a relation is as substantial as the *words* for its terms (pp. 263-264).

We have already remarked on Russell's insistence that relation words cannot be eliminated in favour of relations, but that words

for relations are necessary. Now if Russell is committed to the same linguistic practice as Bradley, how can he avoid the regress ? On both their views, to assert a relational proposition requires using a word for the relation as well as words for the relata. These words must themselves be related, and we seem again to be launched upon a regress of the kind urged by Bradley. Russell noted this fact himself :

> The first step in Bradley's regress does actually have to be taken in giving verbal expression to a relation, and the word for a relation does have to be related to the words for its terms. But this is a linguistic, not a metaphysical, fact, and the regress does not have to go any further (p. 264).

Now how does this solution relate to the *Tractatus* ?

There are no relation words in the " logical notation " recommended by Wittgenstein. Hence formulating a relational proposition in that language does *not* involve " the first step in Bradley's regress " ; and the regress turns out not to be a fact at all, either linguistic or metaphysical. Wittgenstein's recommended " logical symbolism " would appear to provide a threefold answer to Bradley's argument against relations. First, as already explained, its adoption would prevent even the first step of Bradley's regress. Second, because it contains no relation words, its adoption would eliminate the circumstance that " relations are indicated, not by relations, but by words which are as substantial as other words " which Russell held to be " at the bottom of the hopeless muddle which has prevailed in *all* schools of philosophy as to the nature of relations ". Third, Bradley's argument against the reality of relations could not even be formulated in the language recommended (one of Wittgenstein's avowed aims was to avoid errors by employing " a symbolism which excludes them " (3.325)).

Wittgenstein explicitly discusses ontological issues, even though these could not be discussed in the " correct logical notation " mentioned but not used by him. His pronouncement on relations is : " In the atomic fact objects hang one in another, like the [links] of a chain " (2.03). Here we have again the relation-link analogy drawn by Bradley. In a chain successive links are not " united by a link," nor are they well described as being united " by a spatial relation ", as Russell would have it (p. 263). The linkage is not *between* links, but *of* them ; it is not a relation between them, but the way they are related. As DeWitt H. Parker wrote in *Experience and Substance* : " Relations are modes of unification of elements, not further elements requiring unification " (p. 215).

Every use of substantive terms to refer to relations is misleading. Russell was right to insist on this fact. But we should remember that it was earlier insisted on by Wittgenstein.

4. *Objects and Properties*

Traditional metaphysical categories are those of relation, property, and particular. Which of these traditional categories corresponds most closely to Wittgenstein's *objects* ?

It should be clear from the extended discussion in Section 3 that his objects are not relations. The argument can be put very briefly as follows. All objects are represented by names (3.203, 3.22, 3.221), whereas relations are represented not by names, but by relations ; therefore objects and relations are distinct.

It must be acknowledged that there is some evidence that properties *are* conceived as objects. Thus in a parenthetical remark " . . . (This blue colour and that stand in the internal relation of brighter and darker *eo ipso*. It is unthinkable that *these* two objects should not stand in this relation) . . . " (4.123), the term ' object ' is applied to *this blue colour*. But this use of ' object ' for a property is deprecated in the very next sentence of the paragraph cited : " . . . (Here to the shifting use of the words " property " and " relation " there corresponds the shifting use of the word " object ") . . . " (4.123). Thus Wittgenstein acknowledges a certain looseness in his employment of these key terms, and seems to warn us against being misled by his atypical use of the term ' object ' for a property in the passage cited.

Two other propositions are inviting premisses for an argument designed to prove that properties are objects : " It is clear that however different from the real one an imagined world may be, it must have something—a form—in common with the real world " (2.022). " This fixed form consists of the objects " (2.023). The usual view, I think, is that the total number of particulars in the real world is contingent, and that different possible (imagined) worlds can contain different numbers of particulars, whence particulars cannot be common to all imagined worlds and the real one. So objects, being common to all such worlds, cannot be particulars. Therefore, since they are not relations either, objects must be properties.

The argument of the preceding paragraph is wholly unconvincing. There is no evidence Wittgenstein shared the " usual view " that different possible worlds can contain different particulars. But Wittgenstein definitely asserts that *different possible worlds can contain different properties*. The axiom of reducibility

(*12.1 of *Principia Mathematica*) asserts the existence not of particulars but of first-order functions or properties. Hence when Wittgenstein writes that, " We can imagine a world in which the axiom of reducibility is not valid " (6.1233), his words surely imply that properties are not the same in all imagined worlds as in the real world. But since objects are the same in all imagined worlds, it follows that objects are not properties. Since objects are not relations either, it follows by elimination that objects must be particulars.

There is overwhelming evidence that objects are particulars rather than properties. I cite first what might be called *symbolic* evidence. " Thus a proposition ' *fa* ' shows that in its sense the object *a* occurs, two propositions ' *fa* ' and ' *ga* ' that they are both about the same object " (4.1211). Here the object *a* is represented by " *a* ", which usually serves as an individual constant, not as a predicate or property symbol. Again, " So the variable name ' *x* ' is the proper sign of the pseudo-concept *object*. . . . For example in the proposition ' there are two objects which . . . ', by ' ($\exists x, y$) . . . ' . . . " (4.1272). Here the word " object " is symbolized by means of individual variables, not by means of predicate or property variables. Wittgenstein wrote further that " The names are the simple symbols, I indicate them by single letters (x, y, z). The elementary proposition I write as function of the names, in the form ' fx ', ' $\phi (x, y)$ ', etc. . . . " (4.24). Here names (of objects) are symbolized by individual variables rather than predicate or property variables. These cited passages seem to me to provide a kind of " symbolic " evidence that objects are not properties.

Another kind of evidence for the distinctness of objects and properties is furnished by the paragraph : " The substance of the world *can* only determine a form and not any material properties. For these are first presented by the propositions—first formed by the configuration of the objects " (2.0231). The sharply contrasted terms " form " and " material properties " are not explicitly defined, but it is suggested that " formal " has the sense of " logical " : " The fact that the propositions of logic are tautologies *shows* the formal—logical—properties of language, of the world " (6.12, see also 6.1224, 2.0141, 2.033, 4.126). The contrasted " material properties " can best be understood, therefore, as contingent or empirical properties. Now objects are not forms or formal properties, because objects can be represented (3.22, 3.221, 3.203, 4.0312) whereas formal properties can not be represented—" To be able to represent the logical form, we should have to be able to put ourselves with the propositions outside

11

logic, that is outside the world " (4.12, see also 4.121, 4.0312). But objects cannot be material properties either. The second sentence of 2.0231 quoted at the start of this paragraph asserts that material properties are " first formed by the configuration of the objects ". Now if objects *were* material properties, that passage would assert material properties to be first formed by the configuration of material properties, themselves first formed by the configuration of material properties, and so on. Here, it seems to me, would be a regress more vicious than any ever dreamed of by Bradley. The alternative to the vicious regress is to deny that objects can be material properties. And from these considerations it follows that objects cannot be properties, either formal or material.

Perhaps more decisive evidence that objects are not properties can be assembled as follows. In the first place, " The object is simple " (2.02), " Objects . . . cannot be compound " (2.021). But *some* properties are definitely compound. .The most that might be claimed, therefore, is that *some* properties are objects, namely, the simple ones. But symbols that appear to be names of the simplest properties cannot occur in elementary propositions. Wittgenstein proclaimed a radical pluralism, insisting that " Atomic facts are independent of each other " (2.061), and that " . . . It is possible for all combinations of atomic facts to exist, and the others not to exist " (4.27, see also 2.062). Since " The simplest proposition, the elementary proposition, asserts the existence of an atomic fact " (4.21), it follows that " It is a sign of an elementary proposition, that no elementary proposition can contradict it " (4.211, see also 5.134). Now what shall we say of two propositions, one asserting a given point in the visual field to be red, the other asserting it to be blue ? If *any* properties are simple, specific colours ought to be counted among the simplest. If objects are (simple) properties, and elementary propositions consist of names of objects (4.22, 3.202, 3.203), then the two propositions mentioned must be elementary propositions. But *can* they both be true ? Wittgenstein's answer is unequivocal : " For two colours, *e.g.* to be at one place in the visual field, is impossible, logically impossible, for it is excluded by the logical structure of colour. . . . (It is clear that the logical product of two elementary propositions can neither be a tautology nor a contradiction. The assertion that a point in the visual field has two different colours at the same time, is a contradiction) " (6.3751). It follows that colour predications are *not* elementary propositions, and the implication seems clear that objects are not properties.

Further evidence that his objects are particulars rather than properties is provided in Wittgenstein's later *Philosophical Investigations*, where after quoting the *Theaetetus* doctrine that " primary elements " can only be named, he adds " Both Russell's ' individuals ' and my ' objects ' (*Tractatus Logico-Philosophicus*) were such primary elements " (21e).

Since objects are neither relations nor properties, if they correspond to any of the traditional categories they must be particulars. Our earlier distinction between formal (logical) and material (contingent) properties permits three different kinds of particulars to be distinguished. First we note that possession of a material property implies possession of a formal property, but not conversely. If a particular possesses a material property it must also possess the formal property of being capable of possessing that material property. But it is logically possible for a particular to possess a formal property, say the capability of being (materially) related to other particulars, without possessing any material property at all. Now we define an *absolutely bare* particular as one possessing neither formal nor material properties, a *bare* particular as one possessing formal but no material properties, and a *qualitied* particular as one possessing both formal and material properties.

With the possible exception of Parmenides, I know of no historical philosopher who discussed absolutely bare particulars. In the *Timaeus* Plato's receptacle is bare but not absolutely bare : although " formless " or " devoid of any particular form " it has the capability of receiving form (50-51). Certainly Wittgenstein's objects are not absolutely bare ; they have " internal " properties (2.01231) or " forms " (2.0141), where the form is a possibility (2.0141) and therefore logical. His objects are therefore either qualities or bare particulars.

It must be admitted that several of Wittgenstein's remarks suggest that objects have " external " properties as well as " internal " ones (2.01231, 2.0233, 4.023). Despite the difficulty of dealing with such passages, there seems to me to be overwhelming evidence that he regarded objects as bare particulars, having no material properties whatever.

In the first place, Wittgenstein explicitly denies that objects can have properties. His assertion that " objects are colourless " (2.0232) must be understood as synecdochical, for the context makes clear that he is not interested in denying colour qualities only, but all qualities or " material properties " (the term first appears in the immediately preceding paragraph (2.0231)).

Further evidence for the bare particularity of objects is furnished by Wittgenstein's assertions that states of affairs (complexes, facts) can be described (3.144, 3.24, 4.023) but that objects can only be named (3.221). For if an object *had* a property, that would be a fact whose assertion would constitute a *description* of that object. But objects can not be so described, whence it follows that objects have no properties.

A third argument for the bareness of particulars can be set forth as follows. Propositional signs are analyzable (3.25), whereas names or simple signs are not (3.26), hence all propositional signs are composite (4.032), and every proposition must contain at least two elements. Since elementary propositions and the facts they assert must possess " the same logical (mathematical) multiplicity " (4.04), every fact must contain at least two elements. But the elements of an atomic fact are objects (2.01, 2.0272), and since objects are particulars, every fact must involve at least two particulars. Now if an object had a material property, *that* it had the property would be a fact involving only one particular. But no fact can involve just one particular, hence no object can have any material property, and all particulars are bare.

A fourth reason for taking the particulars to be bare is provided by Wittgenstein's remark in the *Philosophical Investigations* that his objects in the *Tractatus* were primary elements like those described in the *Theaetetus* (21e).

A consequence of Wittgenstein's doctrine that objects are bare particulars is that " Sophia hates Amos " cannot be an elementary proposition in an " adequate notation ". Neither Sophia nor Amos are bare particulars, hence they are not objects and " Sophia " and " Amos " are not names, whence it follows that " Sophia hates Amos " does not have names as its elements and is therefore not an elementary proposition.

All atomic facts are relational, and no elementary proposition in a " logical notation " can predicate a property of any object. Hence Mrs. Daitz is right in remarking : " . . . the notion that elements of the sign stand for elements in the signified . . . " starts us on " . . . the road that will end at the bare particular " (p. 187). But since Wittgenstein accepts objects as bare particulars, Mrs. Daitz's remark is scarcely a *reductio ad absurdum* of his picture theory of meaning—as it seemed intended to be. As bare particulars, objects can form the substance of the world (2.021), but not in the Aristotelian sense in which substances have material properties. Wittgenstein's objects are substantial in the later sense of substrata, and correspond more closely to Aristotle's prime matter than to his primary substances.

Material properties, then, are never properties of objects, but belong only to complexes. Russell's remark on this point in *An Outline of Philosophy* is extremely helpful :

> . . . the sort of adjective we can know, such as " blue " and " round ", will not be applicable to particulars. They are therefore analogous to the adjective " populous " applied to a town. To say " this town is populous " means " many people live in this town ". A similar transformation would be demanded by logic in all the adjectives and relations we can know empirically (268).

In the same passage Russell asserts that " True adjectives . . . will require particulars for their terms . . . " but there he is definitely in disagreement with Wittgenstein, of whose particulars *no* adjectives can be truly predicated. For a logical form cannot be represented at all (4.12, 4.121), hence cannot be predicated of anything ; and particulars have no material properties, hence no material properties can be truly predicated of them.

Although the present (partial) interpretation of the *Tractatus* runs counter to more usual interpretations, there seem to me to be strong arguments in its favour. They may not be decisive, but those arguments must be answered by proponents of alternative interpretations. I hope that this essay will stimulate those who disagree to re-examine the *Tractatus* from a new and different point of view.

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Further evidence for the bare particularity of objects is furnished by Wittgenstein's assertions that states of affairs (complexes, facts) can be described (3.144, 3.24, 4.023) but that objects can only be named (3.221). For if an object *had* a property, that would be a fact whose assertion would constitute a *description* of that object. But objects can not be so described, whence it follows that objects have no properties.

A third argument for the bareness of particulars can be set forth as follows. Propositional signs are analyzable (3.25), whereas names or simple signs are not (3.26), hence all propositional signs are composite (4.032), and every proposition must contain at least two elements. Since elementary propositions and the facts they assert must possess " the same logical (mathematical) multiplicity " (4.04), every fact must contain at least two elements. But the elements of an atomic fact are objects (2.01, 2.0272), and since objects are particulars, every fact must involve at least two particulars. Now if an object had a material property, *that* it had the property would be a fact involving only one particular. But no fact can involve just one particular, hence no object can have any material property, and all particulars are bare.

A fourth reason for taking the particulars to be bare is provided by Wittgenstein's remark in the *Philosophical Investigations* that his objects in the *Tractatus* were primary elements like those described in the *Theaetetus* (21e).

A consequence of Wittgenstein's doctrine that objects are bare particulars is that " Sophia hates Amos " cannot be an elementary proposition in an " adequate notation ". Neither Sophia nor Amos are bare particulars, hence they are not objects and " Sophia " and " Amos " are not names, whence it follows that " Sophia hates Amos " does not have names as its elements and is therefore not an elementary proposition.

All atomic facts are relational, and no elementary proposition in a " logical notation " can predicate a property of any object. Hence Mrs. Daitz is right in remarking : " . . . the notion that elements of the sign stand for elements in the signified . . . " starts us on " . . . the road that will end at the bare particular " (p. 187). But since Wittgenstein accepts objects as bare particulars, Mrs. Daitz's remark is scarcely a *reductio ad absurdum* of his picture theory of meaning—as it seemed intended to be. As bare particulars, objects can form the substance of the world (2.021), but not in the Aristotelian sense in which substances have material properties. Wittgenstein's objects are substantial in the later sense of substrata, and correspond more closely to Aristotle's prime matter than to his primary substances.

Material properties, then, are never properties of objects, but belong only to complexes. Russell's remark on this point in *An Outline of Philosophy* is extremely helpful :

> . . . the sort of adjective we can know, such as " blue " and " round ", will not be applicable to particulars. They are therefore analogous to the adjective " populous " applied to a town. To say " this town is populous " means " many people live in this town ". A similar transformation would be demanded by logic in all the adjectives and relations we can know empirically (268).

In the same passage Russell asserts that " True adjectives . . . will require particulars for their terms . . . " but there he is definitely in disagreement with Wittgenstein, of whose particulars *no* adjectives can be truly predicated. For a logical form cannot be represented at all (4.12, 4.121), hence cannot be predicated of anything ; and particulars have no material properties, hence no material properties can be truly predicated of them.

Although the present (partial) interpretation of the *Tractatus* runs counter to more usual interpretations, there seem to me to be strong arguments in its favour. They may not be decisive, but those arguments must be answered by proponents of alternative interpretations. I hope that this essay will stimulate those who disagree to re-examine the *Tractatus* from a new and different point of view.

University of Michigan.

II.—PROPER NAMES

By John R. Searle

Do proper names have senses ? Frege [1] argues that they must
have senses, for, he asks, how else can identity statements be
other than trivially analytic. How, he asks, can a statement of
the form a = b, if true, differ in cognitive value from a = a ?
His answer is that though " a " and " b " have the same referent
they have or may have different *senses*, in which case the state-
ment is true, though not analytically so. But this solution
seems more appropriate where " a " and " b " are both non-
synonymous definite descriptions, or where one is a definite
description and one is a proper name, than where both are proper
names. Consider, for example, statements made with the follow-
ing sentences :

(a) " Tully = Tully " is analytic.
But is
(b) " Tully = Cicero " synthetic ?
If so, then each name must have a different sense, which seems at
first sight most implausible, for we do not ordinarily think of
proper names as having a sense at all in the way that predicates
do ; we do not, *e.g.* give definitions of proper names. But of
course (b) gives us information not conveyed by (a). But is this
information about words ? The statement is not about words.

For the moment let us consider the view that (b) is, like (a),
analytic. A statement is analytic if and only if it is true in
virtue of linguistic rules alone, without any recourse to empirical
investigation. The linguistic rules for using the name " Cicero "
and the linguistic rules for using the name " Tully " are such that
both names refer to, without describing, the same identical
object ; thus it seems the truth of the identity can be established
solely by recourse to these rules and the statement is analytic.
The sense in which the statement is informative is the sense in
which any analytic statement is informative ; it illustrates or
exemplifies certain contingent facts about words, though it does
not of course describe these facts. On this account the difference
between (a) and (b) above is not as great as might at first seem.
Both are analytically true, and both illustrate contingent facts
about our use of symbols. Some philosophers claim that (a) is

[1] *Translations from the Philosophical Writings of Gottlob Frege*, edited by
Geach and Black, pp. 56 ff.

fundamentally different from (b) in that a statement using this form will be true for any arbitrary substitution of symbols replacing " Tully ".[1] This, I wish to argue, is not so. The fact that the same mark refers to the same object on two different occasions of its use is a convenient but contingent usage, and indeed we can easily imagine situations where this would not be the case. Suppose, *e.g.* we have a language in which the rules for using symbols are correlated not simply with a type-word, but with the order of its token appearances in the discourse. Some codes are like this. Suppose the first time an object is referred to in our discourse it is referred to by " x ", the second time by " y ", etc. For anyone who knows this code " $x = y$ " is trivially analytic, but " $x=x$ " is senseless. This example is designed to illustrate the similarity of (a) and (b) above ; both are analytic and both give us information, though each gives us different information, about the use of words. The truth of the statements that Tully $=$ Tully and Tully $=$ Cicero both follow from linguistic rules. But the fact that the words " Tully $=$ Tully " are used to express this identity is just as contingent as, though more universally conventional in our language than, the fact that the words " Tully $=$ Cicero " are used to express the identity of the same object.

This analysis enables us to see how both (a) and (b) could be used to make analytic statements and how in such circumstances we could acquire different information from them, without forcing us to follow either of Frege's proposed solutions, *i.e.* that the two propositions are in some sense about words (*Begriffsschrift*) or his revised solution, that the terms have the same reference but different senses (*Sinn und Bedeutung*). But though this analysis enables us to see how a sentence like (b) *could* be used to make an analytic statement it does not follow that it could not also be used to make a synthetic statement. And indeed some identity statements using two proper names are clearly synthetic ; people who argue that Shakespeare was Bacon are not advancing a thesis about language. In what follows I hope to examine the connection between proper names and their referents in such a manner as to show how both kinds of identity statement are possible and in so doing to show in what sense a proper name has a sense.

I have so far considered the view that the rules governing the use of a proper name are such that it is used to refer to and not to describe a particular object, that it has reference but not sense. But now let us ask how it comes about that we are able to refer

[1] W. V. Quine, *From a Logical Point of View*, esp. chap. 2.

to a particular object by using its name. How, for example, do we learn and teach the use of proper names ? This seems quite simple—we identify the object, and, assuming that our student understands the general conventions governing proper names, we explain that this word is the name of that object. But unless our student already knows another proper name of the object, we can only *identify* the object (the necessary preliminary to teaching the name) by ostension or description ; and, in both cases, we identify the object in virtue of certain of its characteristics. So now it seems as if the rules for a proper name must somehow be logically tied to particular characteristics of the object in such a way that the name has a sense as well as a reference ; indeed, it seems it could not have a reference unless it did have a sense, for how, unless the name has a sense, is it to be correlated with the object ?

Suppose someone answers this argument as follows : " The characteristics located in teaching the name are not the rules for using the proper name : they are simply pedagogic devices employed in teaching the name to someone who does not know how to use it. Once our student has identified the object to which the name applies he can forget or ignore these various descriptions by means of which he identified the object, for they are not part of the sense of the name ; the name does not have a *sense*. Suppose, for example, that we teach the name ' Aristotle ' by explaining that it refers to a Greek philosopher born in Stagira, and suppose that our student continues to use the name correctly, that he gathers more information about Aristotle, and so on. Let us suppose it is discovered later on that Aristotle was not born in Stagira at all, but in Thebes. We will not now say that the meaning of the name has changed, or that Aristotle did not really exist at all. In short, explaining the use of a name by citing characteristics of the object is not giving the rules for the name, for the rules contain no descriptive content at all. They simply correlate the name to the object independently of any descriptions of it."

But is the argument convincing ? Suppose most or even all of our present factual knowledge of Aristotle proved to be true of no one at all, or of several people living in scattered countries and in different centuries ? Would we not say for this reason that Aristotle did not exist after all, and that the name, though it has a conventional sense, refers to no one at all ? On the above account, if anyone said that Aristotle did not exist, this must simply be another way of saying that " Aristotle " denoted no objects, and nothing more ; but if anyone did say that Aristotle

did not exist he might mean much more than simply that the name does not denote anyone.[1] If, for example, we challenged his statement by pointing out that a man named " Aristotle " lived in Hoboken in 1903, he would not regard this as a relevant countercharge. We say of Cerberus and Zeus that neither of them ever existed, without meaning that no object ever bore these names, but only that certain kinds (descriptions) of objects never existed and bore these names. So now it looks as though proper names do have a sense necessarily but have a reference only contingently. They begin to look more and more like shorthand and perhaps vague descriptions.

Let us summarise the two conflicting views under consideration : the first asserts that proper names have essentially a reference but not a sense—proper names denote but do not connote ; the second asserts that they have essentially a sense and only contingently a reference—they refer only on the condition that one and only one object satisfies their sense.

These two views are paths leading to divergent and hoary metaphysical systems. The first leads to ultimate objects of reference, the substances of the scholastics and the *Gegenstände* of the *Tractatus*. The second leads to the identity of indiscernibles, and variables of quantification as the only referential terms in the language. The subject-predicate structure of the language suggests that the first must be right, but the way we use and teach the use of proper names suggests that it cannot be right : a philosophical problem.

Let us begin by examining the second. If it is asserted that every proper name has a sense, it must be legitimate to demand of any name, " What is its sense ? " If it is asserted that a proper name is a kind of shorthand description then we ought to be able to present the description in place of the proper name. But how are we to proceed with this ? If we try to present a complete description of the object as the sense of a proper name, odd consequences would ensue, *e.g.* that any true statement about the object using the name as subject would be analytic, any false one self-contradictory, that the meaning of the name (and perhaps the identity of the object) would change every time there was any change at all in the object, that the name would have different meanings for different people, etc. So suppose we ask what are the necessary and sufficient conditions for applying a particular name to a particular object. Suppose for the sake of argument that we have independent means for locating an object ; then what are the conditions for applying a name to

[1] *Cf.* Wittgenstein, *Philosophical Investigations*, para. 79.

it ; what are the conditions for saying, *e.g.* " This is Aristotle "?
At first sight these conditions seem to be simply that the object
must be identical with an object originally christened by this
name, so the sense of the name would consist in a statement or
set of statements asserting the characteristics which constitute
this identity. The sense of " This is Aristotle " might be,
" This object is spatio-temporally continuous with an object
originally named ' Aristotle ' ". But this will not suffice, for,
as was already suggested, the force of " Aristotle " is greater than
the force of " identical with an object named ' Aristotle ' ", for
not just any object named " Aristotle " will do. " Aristotle "
here refers to a particular object named " Aristotle ", not to any.
" Named ' Aristotle ' " is a universal term, but " Aristotle ", is a
proper name, so " This is named ' Aristotle ' " is at best a necessary
but not a sufficient condition for the truth of " This is Aristotle "?
Briefly and trivially, it is not the identity of this with any object
named " Aristotle ", but rather its identity with Aristotle that
constitutes the necessary and sufficient conditions for the truth
of " This is Aristotle ".

Perhaps we can resolve the conflict between the two views of
the nature of proper names by asking what is the unique function
of proper names in our language. To begin with, they mostly
refer or purport to refer to particular objects ; but of course
other expressions, definite descriptions and demonstratives,
perform this function as well. What then is the difference
between proper names and other singular referring expressions ?
Unlike demonstratives, a proper name refers without pre-
supposing any stage settings or any special contextual conditions
surrounding the utterance of the expression. Unlike definite
descriptions, they do not in general *specify* any characteristics
at all of the objects to which they refer. " Scott " refers to the
same object as does " the author of *Waverley* ", but " Scott "
specifies none of its characteristics, whereas " the author of
Waverley " refers only in virtue of the fact that it does specify a
characteristic. Let us examine this difference more closely.
Following Strawson [1] we may say that referring uses of both
proper names and definite descriptions presuppose the existence
of one and only one object referred to. But as a proper name
does not in general specify any characteristics of the object
referred to, how then does it bring the reference off ? How is a
connection between name and object ever set up ? This, which
seems the crucial question, I want to answer by saying that
though proper names do not normally assert or specify any

[1] " On Referring ", MIND, 1950.

characteristics, their referring uses nonetheless presuppose that
the object to which they purport to refer has certain character-
istics. But which ones ? Suppose we ask the users of the name
" Aristotle " to state what they regard as certain essential and
established facts about him. Their answers would be a set of
uniquely referring descriptive statements. Now what I am
arguing is that the descriptive force of " This is Aristotle " is to
assert that a sufficient but so far unspecified number of these
statements are true of this object. Therefore, referring uses of
" Aristotle " presuppose the existence of an object of whom a
sufficient but so far unspecified number of these statements are
true. To use a proper name referringly is to presuppose the
truth of certain uniquely referring descriptive statements, but
it is not ordinarily to assert these statements or even to indicate
which exactly are presupposed. And herein lies most of the
difficulty. The question of what constitutes the criteria for
" Aristotle " is generally left open, indeed it seldom in fact arises,
and when it does arise it is we, the users of the name, who decide
more or less arbitrarily what these criteria shall be. If, for
example, of the characteristics agreed to be true of Aristotle,
half should be discovered to be true of one man and half true of
another, which would we say was Aristotle ? Neither ? The
question is not decided for us in advance.

But is this imprecision as to what characteristics exactly
constitute the necessary and sufficient conditions for applying a
proper name a mere accident, a product of linguistic slovenli-
ness ? Or does it derive from the functions which proper names
perform for us ? To ask for the criteria for applying the name
" Aristotle " is to ask in the formal mode what Aristotle is ; it is to
ask for a set of identity criteria for the object Aristotle. " What
is Aristotle ? " and " What are the criteria for applying the name
' Aristotle ' ? " ask the same question, the former in the material
mode, and the latter in the formal mode of speech. So if we
came to agreement in advance of using the name on precisely
what characteristics constituted the identity of Aristotle, our
rules for using the name would be precise. But this precision
would be achieved only at the cost of entailing some specific
predicates by any referring use of the name. Indeed, the name
itself would become superfluous for it would become logically
equivalent to this set of descriptions. But if this were the case
we would be in the position of only being able to refer to an
object by describing it. Whereas in fact this is just what the
institution of proper names enables us to avoid and what dis-
tinguishes proper names from descriptions. If the criteria for

proper names were in all cases quite rigid and specific then a proper name would be nothing more than a shorthand for these criteria, a proper name would function exactly like an elaborate definite description. But the uniqueness and immense pragmatic convenience of proper names in our language lie precisely in the fact that they enable us to refer publicly to objects without being forced to raise issues and come to agreement on what descriptive characteristics exactly constitute the identity of the object. They function not as descriptions, but as pegs on which to hang descriptions. Thus the looseness of the criteria for proper names is a necessary condition for isolating the referring function from the describing function of language.

To put the same point differently, suppose we ask, " Why do we have proper names at all ? " Obviously, to refer to individuals. " Yes, but descriptions could do that for us." But only at the cost of specifying identity conditions every time reference is made : suppose we agree to drop " Aristotle " and use, say, " the teacher of Alexander ", then it is a necessary truth that the man referred to is Alexander's teacher—but it is a contingent fact that Aristotle ever went into pedagogy (though I am suggesting it is a necessary fact that Aristotle has the logical sum, inclusive disjunction, of properties commonly attributed to him : any individual not having at least some of these properties could not be Aristotle).

Of course it should not be thought that the only sort of looseness of identity criteria for individuals is that which I have described as peculiar to proper names. Referring uses of definite descriptions may raise problems concerning identity of quite different sorts. This is especially true of past tense definite descriptions. " This is the man who taught Alexander " may be said to entail, e.g. that this object is spatio-temporally continuous with the man teaching Alexander at another point in space-time : but someone might also argue that this man's spatio-temporal continuity is a contingent characteristic and not an identity criterion. And the logical nature of the connection of such characteristics with the man's identity may again be loose and undecided in advance of dispute. But this is quite another dimension of looseness than that which I cited as the looseness of the criteria for applying proper names and does not affect the distinction in function between definite descriptions and proper names, viz. that definite descriptions refer only in virtue of the fact that the criteria are not loose in the original sense, for they refer by telling us what the object is. But proper names refer without so far raising the issue of what the object is.

We are now in a position to explain how it is that " Aristotle " has a reference but does not describe, and yet the statement " Aristotle never existed " says more than that " Aristotle " was never used to refer to any object. The statement asserts that a sufficient number of the conventional presuppositions, descriptive statements, of referring uses of " Aristotle " are false. Precisely which statements are asserted to be false is not yet clear, for what precise conditions constitute the criteria for applying " Aristotle " is not yet laid down by the language.

We can now resolve our paradox : does a proper name have a sense ? If this asks whether or not proper names are used to describe or specify characteristics of objects, the answer is " no ". But if it asks whether or not proper names are logically connected with characteristics of the object to which they refer, the answer is " yes, in a loose sort of way ". (This shows in part the poverty of a rigid sense-reference, denotation-connotation approach to problems in the theory of meaning.)

We might clarify these points by comparing paradigmatic proper names with degenerate proper names like " The Bank of England ". For these latter, it seems the sense is given as straightforwardly as in a definite description; the presuppositions, as it were, rise to the surface. And a proper name may acquire a rigid descriptive use without having the verbal form of a description : God is just, omnipotent, omniscient, etc., *by definition* for believers. Of course the form may mislead us ; the Holy Roman Empire was neither holy, nor Roman, etc., but it was nonetheless the Holy Roman Empire. Again it may be conventional to name only girls " Martha ", but if I name my son " Martha " I may mislead, but I do not lie.

Now reconsider our original identity, " Tully $=$ Cicero ". A statement made using this sentence would, I suggest, be analytic for most people ; the same descriptive presuppositions are associated with each name. But of course if the descriptive presuppositions were different it might be used to make a synthetic statement ; it might even advance a historical discovery of the first importance.

University of Oxford

III.—DETERMINISM VERSUS CONTINUITY IN MODERN SCIENCE

Sufficient Reason for the Lack of Sufficient Causation

By Alfred Landé

WHEN examining the problem of determinism in modern science one may deal with general questions such as whether the world is a tight-fitting mechanism or a gambling device, whether the course of events is predetermined or erratic, predictable by a superior intelligence or unpredictable, and the like. The present article is to analyse only those special phenomena which are known from so-called games of chance, be they ordinary roulette games, microphysical experiments controlled by quantum rules of uncertainty, or those games which insurance companies play with their clients. The question is whether ' accidents ' are accidents, or whether average frequency ratios and fluctuations away from the average are capable of a deterministic explanation in a reasonable fashion. More precisely, when one and the same experimentally prepared situation A is observed to be followed sometimes by a situation B, sometimes by B', or B", etc, at a fixed average frequency ratio (Schema 1(a)),

(*a*) can we hope to reduce this situation to individual deterministic chains (Schema 1(b)) perhaps on the grounds of an improved experimental technique revealing concealed causes,

(*b*) or are such attempts futile in principle regardless of techniques ?

Schema 1

$$
\begin{array}{ll}
 \nearrow B & A \rightarrow B \\
A \rightarrow B' & A' \rightarrow B' \\
 \searrow B'' & A'' \rightarrow B''
\end{array}
$$

(*a*)   (*b*)
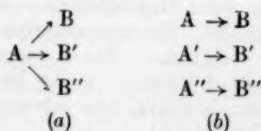
The terms ' hope ', ' reasonable ', ' explain ', etc. indicate that the problem is one of interpretation. As to possible modes of interpretation one may distinguish between hard and soft determinists. Hard determinists would be satisfied only with a reduction of Schema 1(a) to 1(b) in all cases without exception ; they agree with Einstein when he said " I cannot believe that the Lord *ever* plays dice with the world ". Soft determinists also insist that Schema 1(a) can, or ought to be, reduced to 1(b) as a rule, with

one notable exception, however, *viz.* a *random mixing* at one remote instant in the past, so that deterministic chains prevail from there onwards only. Soft determinism was the vogue during the age of classical statistical mechanics ; its slogan was : " We could, if we only would, explain every present statistical situation as the deterministic effect of a corresponding previous statistical situation." This stand may be likened to that of people who enjoy the after-effects of one good drink, but call themselves anti-alcoholics ; it is half-way between the hard determinism of the Einstein variety, and the consistent indeterminism adopted by most quantum physicists today. According to the latter, every single microphysical test experiment A carried out with an ensemble of particles represents an independent game of chance in which the results B, or B', etc., for individual particles are not conditioned by the results of previous tests with the same individual particles.

Microphysical uncertainty is often thought of as being connected with the quantum constant h. Although this is true quantitatively, the difference between classical and quantum games (§ 3) has been widely overrated and obscured by technicalities. In order to prove this contention we shall examine, first, an ordinary game (§ 1) and then proceed, without change of formal reasoning, to a typical ' quantum game ' (§ 2). Thereafter we shall demonstrate that the bewildering lack of determinacy must be accepted as a necessary consequence of a more natural and *intelligible* general empirical postulate, *viz.* that of *continuity of cause and effect* on the macroscopic phenomenological level (§ 4).

§ 1   *Discussion of an Ordinary Game*

The game may consist of dropping balls from a chute onto the edge of a knife. If the chute is aimed at an angle α to the right (left) of the edge, all balls will drop to the right (left) of the knife. Experience shows, however, that there is always a small but *finite* range $\Delta\alpha$ of aim, inside of which not all balls drop to the same side. Instead, for every angle α inside the small range $\Delta\alpha$ one observes a certain frequency ratio between r- and l-balls, a ratio which varies from 100 : 0 to 0 : 100 when the aim α is varied from the one to the other end of the finite range $\Delta\alpha$. It thus appears as though one and the same cause α, if it is within the range $\Delta\alpha$, may produce two different effects, sometimes r and sometimes l, individually at random though controlled by statistical frequency ratios.

This interpretation is usually challenged by the contention that even within the range $\Delta\alpha$ each single r- and l-event has ' in reality '

its distinct r- and l-inducing cause (Schema 1(b)), be it a slight deviation of aim, or a minute dislocation of the knife edge, or a perturbation of the ball on its flight, which in turn are effects of still earlier causes. It is the contention that an individual r-event is but the terminal member of a deterministic chain . . . rrr which goes back to the infinite past.

However, this reduction merely leads to the new question as to why those r- and l-chains, rather than the final r- and l-events, occur at an average frequency ratio, and moreover, why even the fluctuations away from the average conform in all details with the mathematical theory of random (Gauss distribution law of errors, etc.)? This apparent *pre-established harmony* between observed series of events and mathematical error theory is indeed a most unexpected empirical fact ; it was discovered by our ancestors who thereby took an important forward step, from the creed in an erratic and whimsical world towards a cosmos in which even the god of thieves and gamblers was bound to obey rules of fair play and impartial justice. A strange empirical fact calls for an explanation, however, *i.e.* for a reduction to a more intelligible general principle. Instead of appealing to ' justice ' or ' symmetry ', which are but circumlocutions of the observed situation, the only *deterministic explanation* of said pre-established harmony could be the following : Once upon a time there was a *demon* who first started two deterministic r-chains, then an l-chain, then three r-chains. Then realizing that he had given too much preference to r-chains, he started four l-chains in a row, and so forth, deliberately setting up a pseudo-random distribution in order to lure present day observers away from the true deterministic faith, and to make us believe that the Lord plays dice with the world.

However, this make-believe hypothesis can hardly pass for a scientific theory, chiefly because of its *ad hoc* character. Therefore, in the absence of a reasonable deterministic interpretation, we have to concede that the harmony between statistical observation and mathematical theory is a basic feature of the world in which we live, irreducible in principle to a consistent deterministic explanation. The stunning impact of this realization, which is contrary to a three hundred year old tradition, however, may be cushioned by the considerations of § 4.

§ 2   *The Quantum Game*

Let us discuss a slight modification of the ' classical ' game described in § 1. Instead of aiming balls at an angle α onto a knife-edge, consider ' particles ' in a ' state ' S aimed at a ' filter '

where they either are 'passed' or 'rejected'. The terms in quotation marks are to be taken in a most general sense; but Fig. 1 may help to visualize the situation. Among the class of all states S we distinguish the subclass of states A which is passed, and the subclass non-A, written $\overline{A}$, which is rejected by this particular filter. The filter may be called an A-passing filter, or A-filter for short. It separates the subclasses A and $\overline{A}$ from one another, thereby characterizing them as discernible, as entirely different, written $A \neq \overline{A}$.
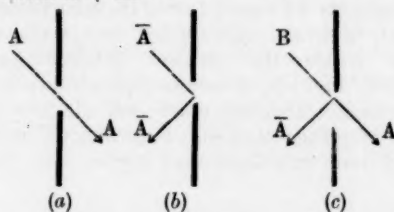

Fig. 1.

Experience shows, however, that there is a third subclass of states intermediate between A (always passed) and $\overline{A}$ (never passed), *viz.* states B which sometimes are passed, sometimes are rejected by the same A-filter, as shown in Fig. 1 (c). States B showing this reaction can neither be equal to A nor entirely different from A. We denote B as of a 'fractional equality' with A, written $B \sim A$. At the same time we may define the quantitative 'fractional degree of equality' between A and B as the statistical passing fraction of B-state particles through an A-filter (and of A-state particles through a B-filter; this *symmetry* condition is empirically confirmed). Fig. 1 (c) represents the 'splitting effect' of quantum physics; Figs. 1 (a) and 1 (b) are but limiting cases of the general phenomenon of a statistical reaction of particles incident in a state B upon a 'filter' or testing instrument in general whereby the latter reacts with 'yes' or 'no'.

The passed and the rejected particles in Fig. 1 (c) are labelled A and $\overline{A}$ respectively in order to describe the further observation that the incident B-state particles, by virtue of being passed (or rejected) by the A-filter, *jump* from their original state B to the states A and $\overline{A}$ respectively and then stay in the newly acquired states. These jumps present us with a strange 'non-classical' or 'quantum' phenomenon. An explanation of this phenomenon, *i.e.* its deduction from a more readily acceptable, more intelligible general principle is obtained by appealing to the

12

postulate of *reproducibility of a measurement* (= test result) which
reads : " Once passed always passed, and once rejected always
rejected " or more explicitly : " A particle, or a mechanical
system in general, which has once responded with ' yes ' (or ' no ')
to a certain yes-or-no test, will respond in the same fashion again
upon a repetition of the test." This requirement implies indeed
that the B-state particles in the act of passing (in being rejected)
must have *jumped* to the new states A (or $\overline{A}$) so that they will pass
(be rejected) in a subsequent A-filter test with certainty again.

The developments of § 1 and § 2 are of the same formal structure;
they differ only in details, right and left being replaced by passed
and rejected. Neither the ' classical ' ball-knife game nor the
' splitting effect ' Fig. 1 (c) of quantum physics can be reduced to
individual cause-effect chains, unless one admits a demon who
feigns a sort of pseudo-random. A certain difference between
classical and quantum-mechanical games will be discussed
presently.

§ 3   One or Many Games of Chance ?

Imagine a great number N of arrows distributed as radii of a
circle in a random fashion. Suppose that this ' star ' of arrows
is viewed through a circular glass A which is divided into three
equal sections A_1 A_2 A_3. Here one can " predict " that each
section will cover N/3 arrows. Also the betting odds of finding an
individual arrow within section A_1 or A_2 or A_3 are 1/3 each. The
predictions and betting odds in this and similar games are
deterministic consequences of the one initial random distribution of
arrows over all directions. And these bets and predictions may
be confirmed simply by viewing the arrows through various in-
struments such as calibrated glasses, without thereby affecting
the state of any arrow so viewed. This is the classical situation.

When the N macroscopic arrows are replaced by micro-arrows
representing the spin directions of N electrons, however, then not
even a demon can distribute them over all directions of a circle
simultaneously. Spin arrows can be laid out only by passing
them through a linear ' slit ' or ' filter ', *e.g.* a magnetic field held
in a certain direction, from which the spins emerge in two opposite
directions, parallel and antiparallel to the field. When the
magnetic field is held in the NS.-direction, the spins line up in the
N.- and S.-direction only. When they are later tested by a
second field, this time of NE.-SW.-direction, each spin will *jump*
from its original N.- or S.-direction either to the NE.- or SW.-
direction. For an individual arrow it is a game of chance
whether it will jump to the NE.- or the SW.-direction ; only the

odds of the various jumps have definite magnitudes. (They are equal to the cosine squares of half the angle between the original and the final direction.) The testing or measuring instrument, in the present instance the magnetic field, plays an active part in compelling the tested particles to jump from their original to one of the several states peculiar to the instrument. Which of the latter states is reached by an individual particle is a matter of probability rather than certainty. Similarly, when a position meter has located a particle at a certain point of co-ordinate x or within a small range Δx, and when this observation is followed by a test of the x-momentum of the same particle, then it is a matter of probability which of the various momentum values will actually turn up in such a test. And no experimental arrangement can put particles into a state with both position and momentum having definite values simultaneously (Heisenberg). That is to say, position and momentum are mutually *incompatible* observables. Similarly, spin orientation along the NS.-direction and along the NE.-SW.-direction are mutually incompatible.

It was indeed a stunning empirical discovery that certain mechanical quantities, such as position and momentum, which were considered as compatible in classical mechanics, turned out to be mutually incompatible. Yet incompatible quantities as such have been known long before quantum physics. Thus, using an example of P. Feyerabend, one cannot convey to a sample of water both a certain value of rigidity and viscosity at the same time, since the latter belongs to the liquid, the former to the frozen state ; measuring the one invalidates the previously measured value of the other. Hence it is sound linguistic advice not to speak of, and not to imagine, a sample of water *possessing* certain values of rigidity and viscosity at the same time. But such advice is hardly a *philosophical* achievement which unfolds a new era in the theory of knowledge. Likewise, telling us not to speak of, or trying to imagine, a particle possessing a certain value of position and of momentum at the same time is sound advice. But the present writer is unable to see why this is hailed as the most revolutionary innovation in the *philosophy* of science of the century. There is no doubt that the discovery of the incompatibility of certain *mechanical* quantities is of greatest importance for the science of mechanics. But presenting it as world-shaking philosophical revelation seems to be slightly exaggerated.

§ 4   Indeterminacy, a Consequence of Continuity

The breakdown of determinism in the face of observational series in harmony with mathematical error theory is certainly a blow to

our ingrained ways of thinking in natural science. However, " a conceptual scheme is either modified or replaced by a better one, never abandoned with nothing to take its place " (Conant). It is fortunate, therefore, that the shock of having to abandon determinism can be softened by the realization that a certain fundamental experience, familiar to us since childhood days, compels us to accept statistical laws irreducible to individual determinism. We refer to the Leibnitzian *principle of continuity of cause and effect* which reads : " An infinitely small change of cause never produces a finite change of effect."

The implications of the continuity postulate for the game of dropping balls onto a knife-edge are obvious. Under the continuity postulate it is ' unphysical ' to expect an *abrupt* change, from all balls dropping to the right to all balls dropping to the left, upon a *gradual* shift of aim from right to left. Abrupt changes may be imagined theoretically, but they do not occur in nature. Rather, when physical balls are aimed through a physical chute at a physical knife, then for the sake of continuity we have to expect that there be a finite range $\Delta\alpha$ of the angle α of aim, so that within this finite range a gradual change of the $r : l$ ratio from $100 : 0$ to $0 : 100$ per cent. will occur. The only conceivable manifestation of such a gradual change is the occurrence of intermediate *statistical* ratios. Indeterminacy controlled by statistical laws can thus be seen as a necessary consequence of the postulate of continuity for cause and effect, and thereby become more palatable.

According to a similar consideration one will have to *expect* those phenomena of quantum physics which in § 2 were simply *described* as the ' splitting effect '. Indeed, suppose the state B has been ascertained as *equal* to A by experiments in which B-state particles pass an A-filter, and A-state particles pass a B-filter, as in Fig. 1 (*a*). Suppose now that the state B of the incident particles is modified to an amount however small, or infinitely little in the limit. To expect that the same A-filter will now suddenly stop all incident particles would constitute a violation of the postulate of continuity of cause and effect. There rather ought to be a gradual change from the reaction of Fig. 1 (*a*) to that of 1 (*b*) *via* intermediate cases, *i.e.* from equality $B = A$ to total inequality $B \neq A$, via various degrees of ' fractional equality ' $B \sim A$. But the only conceivable reactions intermediate between those of Figs. 1 (*a*) and 1 (*b*) are those of Fig. 1 (*c*). Between the reaction *yes* of Fig. 1 (*a*) and *no* of Fig. 1 (*b*) is reaction *undetermined* (Reichenbach). But instead of introducing a three-valued logic, we can settle for a statistical ratio between *yes* and

no, as an inevitable physical consequence of the postulate of continuity for cause and effect, without having to accept a new logic.

It was seen above that the postulate of reproducibility of a measurement (= test result) leads to the further prediction that there ought to be jumps from one to other states in test experiments. Going still further, the author has attempted to develop the whole body of quantum rules and prescriptions together with the conceptual framework of the Born-Heisenberg statistical theory from a few empirical postulates of an elementary and almost common-sense character (continuity, reproducibility, symmetry, of probability, etc.). Such a deduction of the bewildering " principles " of the quantum theory is of course a challenge to the Copenhagen School of theoretical physics whose followers claim that Niels Bohr's complementarity principle represents the ultimate bottom of theoretical analysis, beyond which to penetrate would be a violation of the faith.

Ohio State University

IV.—TIME AND THEORY IN SOCIAL ANTHROPOLOGY

By Ernest Gellner

Philosophers have recently discussed whether an event may be caused by another later than itself. I shall discuss issues connected with a less obvious question—namely, whether events may be explained by *earlier* events. What makes this particularly interesting is that it has arisen, in a lively way, not from a marginal, doubtful and above all *ex officio* paradoxical pursuit such as research, nor in formal philosophy, but from the day to psychical day work of a respectable empirical discipline, social anthropology.

I propose to discuss a central theme—perhaps the central theme—of a recent book by Mr. E. R. Leach, entitled *Political Systems of Highland Burma*, with the subtitle *A Study of Kachin Social Structure*, published in 1954 with a foreword by Professor Raymond Firth. This book was, rightly as far as I am qualified to judge, acclaimed by social anthropologists as one of the best and most important monographs to appear since the war. Its author is a well known and respected professional social anthropologist, who has received his training from men who are acknowledged leaders in that field ; briefly he is himself not merely an initiated but an important member of the tribe of social anthropologists. I mention these facts to indicate that an examination of his thought will present a picture, not indeed necessarily of just what anthropologists think, but of the general manner in which they are liable to think and of the problems they face.

The theme which I shall discuss is certain considerations Leach puts forward concerning the relationship between the anthropologist's thought and the reality he is investigating. This, quite plainly, is a philosophical theme. The dissection of what Leach has to say on this matter is of interest to philosophers in that it will show the very old epistemological argument reappearing in a new and concrete setting, and re-appearing so to speak spontaneously. Leach is acquainted with philosophy and the bibliography of his work on Burmese hill tribes contains, oddly enough, the names of Vaihinger, Bertrand Russell and Wittgenstein ; but it seems to me on internal evidence that the epistemological doctrine under discussion was suggested spontaneously in the course of grappling with his ethnographic material, and by his interpretation of anthropological theory—not by philosophers.

This dissection might, however, be of interest also to anthropologists, showing how very general considerations, which in the past have arisen from analysing the relation between thought and things as such, have entered anthropological theory in the misleading guise of something specially intimately connected with anthropological material.

Before proceeding any further it is necessary to tell the philosophical reader certain things about the intellectual milieu in which anthropologists operate. Anthropologists distinguish between ethnography and anthropological theory. Very roughly ethnography is supposed to mean the brute facts about a society, whereas theory consists of some kind of conclusions drawn from these facts, conclusions which need not be obvious. Indeed it is preferred if they are not obvious. An anthropologist naturally has a greater sense of achievement if he succeeds in showing that something which seems to perform one role in fact also or primarily performs another, or that something which seems to perform no role at all or one that only makes sense in the idiom of the society concerned (but not objectively), does perform an intelligible role after all. More merit attaches to showing that a feud really contributes to the coherence of the group, or that the religious ritual has important economic consequences, etc., than to saying that the overtly or apparently economic really is such, or that the apparently pointless ritual really has no point at all. This natural and legitimate preference for discovering the new rather than the obvious may of course degenerate into a pursuit of paradox, or an exaggeration of the way in which various human activities are interrelated. But still, anthropology is, more perhaps than other sciences, the study of how things are more and other than they seem.

Anthropologists tend to think of themselves as distinguished from non-anthropologists by their ability to draw such conclusions and to draw them correctly, rather than by being people who know, preferably by direct contact, some very odd facts about odd peoples, which is the way the general public tends to think of them. I shall not here go into the quite difficult question of the logical nature of those conclusions. A typical anthropological study is a kind of inverted sermon. Whilst a sermon begins with a text and proceeds to illustrate it, an anthropological study begins with empirical material and proceeds to extract from it some interesting proposition.

There are analogies between the Functionalist anthropologist and the linguistic philosopher. The latter, having abjured the doctrines leading to translation, reduction, of actually made

statements to simpler models, seeks instead their meaning in
their use (function ?) in the actual situations in which they occur.
The anthropologist, debarred by his calling from discounting
primitive religion, magic, etc., simply as error, mistaken tech-
nology, etc., again seeks their " meaning " in their function (use ?)
in their social context.   Ironically enough, at the very time when
philosophers, in analysing and classifying types of " meaning ",
are turning to the actual context and activity for a clue, we shall
see one whose professional concern is with just those activities
and contexts turn to philosophy and what is perhaps a slightly
dated theory of meaning (parallelism, mirroring) for aid.

Functionalism, in the broad sense in which it can be said that
most British social anthropologists are Functionalists, consists
essentially of seeking explanations in the interrelations of various
features of a society with each other, in their reciprocal usefulness,
as opposed to seeking them in their genesis.   In practice this
approach is combined with an insistence on thorough and intimate
field work, on not taking either information or explanation offered
by members of a society at its face value but on the contrary
checking both by close observation or even participation.   One
should add that in practice, especially of late, this approach is
also combined with careful collection of evidence about genesis
when such evidence is available ; but when this is done it is not
clear in view of the anthropologists' general outlook just what the
relevance of this information is to be, as Leach points out.

Naturally, Functionalism can be formulated both as a regula-
tive and as a constitutive principle, as a method or as a doctrine.
Its merits in the first form are as great as its defects in the second
are obvious.   (This verdict is perhaps analogous to Kant's
critique of teleology : and indeed, teleology is a kind of cosmic
Functionalism.)   But anthropologists do not necessarily or *ex
cathedra* distinguish between the two aspects : they may even
deliberately abstain from doing so for teaching purposes.   The
preoccupation with the truth that everything is, to some extent,
functional, has obscured the even more important truth that some
things are *far* more so than others.   If Functionalism is to be
taken seriously as a doctrine rather than as a method, one can
only say that it fluctuates between two extreme formulations.
At one end of the scale there is the doctrine that societies are
perfect organisms or mechanisms, whose every custom, institution,
etc., is ideally adjusted.   At the other end of the scale there is the
doctrine that customs, institutions, etc., all have many com-
plicated and indirect consequences, and that in any society that
has managed to survive these effects cannot be altogether bad.

The first interpretation is grotesque, and the second is trivially
true and probably an understatement. The truth must lie some-
where in between, but Functionalism as a doctrine is semi-vacuous
in that it fails to say just where. Or, to put this another way,
Functionalism implies that the features of a society must be what
they are, given that its other features are what *they* are. Now,
quite apart from the fact that only structural and not cultural
facets of a society are supposed to be so uniquely determined, the
question arises—how many of the variable must be given before
the others can be uniquely inferred ? Can the whole animal be
re-constructed from one bone, or must all bones but one be given
before the last can be deduced?[1]

Anthropological custom—one might say ritual—is to spend
about two years acquiring intimate knowledge concerning a
society and then to " write up " the material that has been
gathered. If the published material is to earn the label " anthro-
pology " rather than be relegated to " mere ethnography ",
it must as indicated contain " theoretical conclusions ". Now
it may strike philosophers as odd that conclusions should be
expected from single instances. The final justification of this
procedure cannot be attempted here, but it is worth while
making certain observations about it. The practice raises few
logical problems when the theoretical conclusions are negative ;
as, for instance, a general theory of promiscuity amongst primi-
tives is refuted by the demonstration of marriage and family
rules amongst people who by all other criteria fall amongst the
most primitive societies extant. The habit of drawing con-
clusions from a single field study may have been stimulated by
the fact that early studies were sometimes most interesting in as
far as they constituted such demolitions of general theories.
Another point is that two years in the field and *n* years writing
up the material is a long time, during which the anthropologist
has much time to think ; he cannot humanly be expected to
restrain his theoretical ingenuity and inventiveness simply in
deference to a textbook methodology that warns against premature
generalisation. The gathering of empirical material tends to
take longer in the social sciences than it does in the natural
ones, and the customary ratio between the quantity of fact and
theory must adapt itself to this, if only for such purely pragmatic
reasons.

Furthermore, it may be that though *a* society is in one sense
a single instance, in another it is a large class of events, and that

[1] *Cf.* Ronald Fletcher, " Functionalism as a Social Theory ", *The
Sociological Review*, July 1956.

the anthropologist's theory has this large class as its field
rather than all of them seen collectively as "one society".
Other possibilities are that the theory just is a frankly rash
extrapolation intended to serve as a guide to other studies and
expecting to be knocked down by their findings ; or on the con-
trary the theory is not even expected to hold outside the bounds
of the society whose facts it explains. This may be true without
even leading one to some doctrine that anthropological under-
standing is a case of *verstehen* of a unique object.

<p style="text-align:center">*   *   *</p>

The book under discussion is a case in point. Its author
presents a quantity of ethnographic material which, however, he
does not consider important as such, if only because, according to
him, most of it had been published before. Through it, however,
and above all through difficulties encountered when describing
and explaining it, he comes to certain conclusions which he con-
siders of importance for the development of the subject, and it is
these conclusions which interest me. I shall let Leach state
these conclusions in his own words, though I am here editing
them by selection, re-ordering and occasional italicising :

> The social anthropologist . . . endeavours to employ a ter-
> minology which is completely unambiguous. He therefore adopts
> . . . a language of special terms which have no meanings at all
> other than that with which the scientist endows them. Such
> expressions as exogamy . . . etc. . . . used by anthropologists
> . . . mean just what the anthropologist says they mean, neither
> more nor less. *Consequently* structural systems as described by
> anthropologists are always static systems (p. 103).
> Is it then possible to describe at all, by means of ordinary
> sociological categories, societies which are not assumed to be in
> stable equilibrium ? (p. 4).
> The structures which the anthropologist describes are models
> which exist only as logical constructions in his own mind. What
> is more difficult is to relate such abstractions to the data of
> empirical field work. . . . (For) Real societies exist in time and
> space. The . . . situation does not build up into a fixed en-
> vironment, but into a constantly changing environment. Every
> real society is a process in time (p. 5).
> When an anthropologist attempts to describe a social system
> he *necessarily* describes only a model of the social reality. This
> model represents in effect the anthropologist's hypothesis about
> "how the social system works". The different parts of the
> model system *therefore necessarily* form a coherent whole—it is a
> system in equilibrium. . . . on the contrary the reality situation
> is in most cases full of inconsistencies . . . (p. 8).

In practical field work situations the anthropologist must always treat the material of observation as if it were part of an overall equilibrium, otherwise description becomes almost impossible. All I am asking is that the fictional nature of this equilibrium be frankly recognised (p. 285).

The italics are mine throughout. Where Leach italicised something I have left his italics out, but I do not think this alters his meaning significantly at any of these points.

Further examples could be given, but I do not think it necessary. I have picked out those passages which illustrate Leach's central argument without referring to the ethnographic material, which is far too complicated to be re-stated here.

A philosopher reading the above quotations will rapidly recognise the argument—indeed he will recognise it as an old friend, stated here with admirable pungency and brevity. Leach is, in effect, saying ; concepts have determinate meaning, and being thus determinate must therefore be static. But reality is not static. Therefore concepts cannot correspond to reality. He seems to feel that concepts describing change would themselves have to be changing concepts. A much earlier philosopher might have put his point thus : you cannot enter the *same* society twice ; nay, you cannot even do it once. Leach goes on to conclude therefore, concepts do not refer to reality directly, but only through an intermediary, a schizophrenic broker (to borrow Professor Ryle's expression used in another context) who has the ability to look both ways. As changing and disorderly reality cannot be the objects of determinate and neat concepts directly, it can be their object only indirectly through a Third Man. This latter assistant Leach calls a *model*.

The first comment one can make on Leach's argument is that it is valid. If Leach and perhaps other anthropologists wish to speak in this way, they may. But they should consider the consequences. For one thing, not merely will saying things like " Kachin society is patrilineal " be a case of referring to " models " ; but so will saying " It is a nice day today "—especially in England—or " I am sitting at my desk "—why, I fidgeted whilst I typed it, an instance of change unreflected in the concept " sitting " ! All these will also be cases of referring " merely " to " models in one's mind ". *All* references to reality will be mediated by models. Models will be an inescapable iron curtain between all our words and the things they refer to. We have all been busy constructing such models of reality all our lives, rather as Monsieur Jourdain has been speaking prose. The second consequence they will have to face is that they risk

being misled into thinking that there are entities called models over and above meaningful words on the one hand and things and events on the other, for Leach does not mean actual physical models such as an engineer might build. He uses " models " in a sense such that every time a precise concept refers to changing reality, there is, necessarily, a " model ". Moreover, all concepts must have some precision and all reality undergoes some change. . . .

It seems in fact that what Leach has done is to apply to social anthropology the general argument which from the premiss that words must have determinate meanings in order to mean any-thing concludes that they are therefore a kind of static thing, and then noticing that reality is not static, finally concludes that words can either not refer to reality at all or only through some further intermediary.

*   *   *

To say this about Leach, however, is not to explain his argu-ment sufficiently. Leach, like reality, is more complicated. As I wish to discuss Leach and not a model of Leach—an ambition which I do not think the very nature of knowledge will frustrate— I must add certain complicating touches to the above sketch. These complicating touches will have the interest of showing how the purely philosophical argument becomes enmeshed in considera-tions which are specifically connected with anthropological concepts and not merely concepts in general, and with the social organisation of Burmese hill tribes rather than with objects of knowledge in general.

It is not wholly clear to me after reading the book whether the argument is meant to follow from something about concepts in general, or about certain current anthropological concepts in particular. Secondly, it is not clear whether the difficulty which Leach is solving arises when *any* reality is related to concepts, or whether it arises only when unstable, changing things are so related. I think that in connexion with both these issues both the general and the more specific interpretation is possible, and perhaps Leach himself is not altogether clear as to which one he is putting forward. Some of the quotations given above support the " general " interpretation. For instance :

" . . . expressions . . . mean just what the anthropologist says they mean. . . . Consequently . . . systems as described by anthropologists are always static systems." (But) " Every real society is a process in time."

In order to see the " specific " interpretation of Leach's

argument, one must look at least briefly at his theory of meaning
and symbolism, his theory of ritual and his theory of social change,
all these being closely related. I shall not go into his theory of
meaning in detail, partly because it would take too long and partly
because an excellent analysis of its already exists.[1]

Roughly speaking, Leach believes that language, systems of
propositions, describe reality in virtue of reflecting it in some
fairly literal sense, in other words he believes in what might be
called the parallelism theory of meaning. This is what we need
know of his philosophy. As a matter of anthropology, he believes
that ritual reflects the society in which it occurs in a similar way.
Furthermore, he seems to equate " social structure " with ideas
in people's minds as to how their society *should* be ideally organ-
ised, with how power should be distributed within it. We thus
get four entities which are parallel or " reflect each other ", or,
in the last case, are actually identical : verbal description of a
society by an anthropologist, ritual in that society, the ideal of
that society as envisaged by its members, and finally the structure
of that society itself. Leach must take a very " dispositional "
view of " ideas " for this last identity to hold, and there is indeed
independent evidence that he does take such a view. This equa-
tion in Leach's terminology is facilitated by the partial overlaps
in the *normal* meanings of the members of the following series :
' Ideal ', ' correct behaviour ', ' expected behaviour ' ' social
structure '. Readers who might boggle at the equation of
' ideal ' and ' structure ' may see its justification in this context.

Now the relevant part of Leach's theory of social change,
though not necessarily of *all* social change, is that it occurs
when two contradictory ideals are present in the minds of various
members of one society or even the same members, and actual
reality then fluctuates between the two ideals. The part of
Leach's theory of social change amongst Burmese hill tribes
that I have given may seem, when stated thus in isolation, to
be tautologous ; for contradictory ideals which at the same time
are the social structure can only explain changes in that structure
in a circular way. In fact, Leach explains the fluctuations in
terms of the detail *operation* of the two structures (which I cannot
reproduce), not merely by saying that there are two. This
fluctuation of the social structure, however, does not require a
similar fluctuation in ritual, despite the fact that ritual was said
to mirror both social structure and ideals ; because, although
ritual is in Leach's view a kind of language which describes
society and can even convey negative assertions and deny that a

[1] *Cf.* a forthcoming article by Mr. Paul Stirling.

given kind of organisation obtains, *it is a sufficiently vague language to incorporate both of two contradictory ideals.* This is the point in Leach's anthropology proper that it was necessary to reach for the purposes of the present argument.[1]

This point can be cited in support of what I called the "specific" rather than the "general" interpretation of Leach. It seems to show that it is not simply the fact that anthropological concepts are concepts that prevent them from faithfully reflecting reality, but the fact that they are precise. It shows this because Leach is now seen to think that ritual can, thanks to its vagueness, mirror even an "inconsistent" and hence fluctuating social structure. One might at this stage remark, rather unkindly, that if lack of vagueness is the only thing standing between some at least amongst anthropological concepts and reality, there should be nothing to worry about. Take a concept such as "equilibrium", or the concept of "solidarity", which according to anthropologists is generated by ritual. In discussing this notion Leach mentions an instance where ritual occurs with allegedly this consequence but where subsequent events force one to say that the solidarity is dissipated immediately after the celebrations that had the function of promoting it. Come to think of it, we are most of us acquainted with this kind of thing from our own experience. . . . Leach seems nevertheless inclined to retain the notion of solidarity for explanatory purposes even for such cases. *If* "solidarity" is really to be an explanatory concept for ritual despite the fact that the ritual and immediate accompanying emotion may be the only manifestation of it, then we should have to draw attention to the fact that Durkheim, who popularised it, was a compatriot of Molière's medico. But it is only fair to add that many anthropological concepts, notably those connected with kinship, are not at all imprecise ; and furthermore that others, such as " witchcraft ", may rightly be

[1] The doctrine that ritual and belief systems mirror the social organisation in which they occur has wide currency amongst social anthropologists. It is, in fact, a very suggestive and illuminating idea, helpful in formulating questions for research and in organising material. As a formally maintained doctrine it has logical defects of which anthropologists may not be sufficiently aware. Mainly : the idea of ' reflection ' or ' mirroring ' presupposes, as a minimum requirement, that there be a one-one correspondence between the two mutually reflecting systems. But neither system—neither social organisation, nor a system of ritual or belief— consists of easily separable, identifiable, countable ' parts '. The principles of individuation of such parts are largely arbitrary. It follows that any anthropologist who wishes can always, as far as this condition goes, with some ingenuity or ruthlessness interpret *any* material so that it fits the ' reflection ' thesis.

vague, and that the attempts of some anthropologists to make them more precise may well be misguided, springing from a failure to appreciate that these concepts should have an " open texture ".

But whether Leach would settle on the general or the specific interpretation of his thesis, the over-all point holds.   If he settled on the general one, the philosophical comments indicated are called for ; if on the other hand he is to be interpreted in the " specific " way, his arguments are still open to the same philosophic criticism, but one could say that he is by misleading means getting at something which is true, namely, that Functionalist *theory* does have some difficulty in incorporating social change. One might go on to say that this theory is absorbed into the concepts occurring in it, though I am not sure to what extent this is so and the extent to which it is so probably varies from concept to concept.   It is true that when equilibrium theory or Functionalist theory comes to be used in its extreme form—when no feature of a society is allowed to be without function and, for instance, it is said that there are no cultural survivals—then Leach's thesis that change is something incompatible with the concepts employed becomes true.   I have heard Functionalist theory used in this way.   Within such a scheme substantial social changes other than exogenous ones are indeed hard to conceive.   Anyone who had taken this kind of outlook seriously would, I think, naturally find himself thinking along lines such as Leach's when faced with similar material.

One way, however, of showing the error of the solution presented by Leach is to point out that it leads to a regress ; if precise concepts cannot reflect shifting reality, why should the model that is intended to mediate between them be able to do so ? Might not a further mediator be required, and so on ?   Leach seems aware of this difficulty and worried by it.   As, however, the " model " in his sense is a mythological entity, it is hard for us or Leach to decide what it can or cannot reflect.

Leach has, incidentally, an ingenious device for getting round the difficulty of how models succeed in reflecting changing societies, arrived at by combining his theory of social change and his theory of meaning.   He advises anthropologists to cope with fluctuating societies by specifying the contradictory ideals that are operating—which can be done through " static " models employing " static " concepts—thereby simultaneously indicating the mechanism of change *and* describing a changing society by means of two unchanging models, thus achieving indirectly what to his mind is (almost or totally ?) impossible to do directly,

namely to describe change. As however, the difficulty may be un-real, the ingenuity of the device for getting round it may be wasted.

Another way of bringing home the fallacy of the central argument might be to show that however much we maltreat concepts to make them reflect more faithfully a chaotic and untidy reality, we shall never have much reason to think that they then reflect reality more faithfully. If we invent rapidly changing concepts to deal with changing societies, or contradictory concepts to cope with societies in conflict, we may find ourselves with an unmanageable language, but we shall still not be able to be sure that the concepts change or internally conflict in just the way that the society is changing or conflicting. Or alterna-tively if we devise concepts as vague as according to Leach the "concepts" of ritual actually are, what will happen to us if society suddenly confutes us by going all orderly and neat ? The answer is of course that no such simple parallelism between concepts and things such as Leach seems to expect is required in the first place, nor does it exist between ritual and social structure, and that consequently no devices are required to solve alleged difficulties arising when the parallelism ceases to hold. The concept of "change", for instance, does not itself change yet it can "reflect" reality as much as the concept "stability" ; neither of the two is any vaguer than the other. It would be mistaken to think that the concept "stability" is itself stable, or indeed unstable, except in the sense that it has a determined and unchanging meaning, and *that* it has to no greater or lesser degree than the concept of "change". Both have an equally good prospect of describing things denoted by them. As far as these formal considerations go, a changing or un-stable society is no whit less knowable than an utterly static one.

Of course there are certain factors which do make it easier to study, describe and understand stable societies than unstable ones. There are the practical advantages of stable societies ; they enable one to settle down, to gather material over a wider time span, to sort out the typical from the deviant—all of which may be impossible in a society in turmoil. The second point is that "society" *means* a *fairly* stable pattern of personal relationships, though not nearly as closely and exclusively as functionalism suggests. But these considerations do not justify the conclusion that change or disorder are undescribable. Even *indescribable* disorders are occasionally described.

Leach in effect sees *other* Functionalists as holding a kind of Platonism. Only the static is properly knowable, it is merely approximated by the empirically real, and moreover the static

is superior not merely for purposes of cognition but also morally and from the viewpoint of social health. Leach's own variant to all this is a kind of Hegelianism : reality changes because it is in conflict, the conflict is a conflict of embodied ideas, and the change and conflict *are* knowable by means of concepts that are themselves in conflict in a parallel way.

*   *   *

Now whilst Leach's central argument seems to me mistaken and unnecessary, the conclusions he draws from it seem to me true. He is attacking a position which should indeed be re-considered, but he is doing it in a misleading way. The correct conclusion is that anthropologists should pay more attention to change and hence to historical evidence which alone provides information over larger time-spans, than at least one of the official theories of what they are doing suggests. The reasons why this con-clusion seems to me valid are different from the ones given by Leach. In order to make them clear I shall try to consider and speculate why, as Leach points out, some British social anthropo-logists tended to be anti-historical or a-historical and what this amounts to. In their hostility to historical explanations anthro-pologists sometimes seem to an outsider to get near to what may be a kind of Russellian paradox. They insist that anthropology differs from " mere " ethnography by also having theory ; on investigation of this theory, called Functionalism, it turns out to be in large part the doctrine that anthropology should be nothing but ethnography. Or rather—*good* ethnography. For Functionalism amounts largely to the recommendation to study societies as they are and " function " rather than speculate about how they came to be. Anthropologists seem to me to exaggerate the sharpness and genuineness of the distinction between brute ethnographic facts and " explanations " which are not historical.

In as far as anthropologists have been anti-historical they may have such an attitude for a number of separate reasons ;

Firstly there is the brute fact that illiterate societies have no records, and that in consequence past social organisation is hard or impossible to reconstruct. The anthropologists' attitude is " Whereof we have no evidence, thereof we must be silent ". This is impeccable, except that the only way evidence may come to light is by speaking first and thus noticing such evidence as might after all exist ; without speaking first in violation of the above principle, possible evidence is not likely ever to be noticed. In any case, this objection to the historical approach to anthropo-logy is not theoretically very interesting, even if correct. This

13

objection has no deeper roots, logically speaking, than an objection
to mapping the craters on the other side of the moon ; we know
that there are some but we are by practical difficulties prevented
from ascertaining just where. It is worth adding that this atti-
tude of anthropologists sprang from a healthy reaction against
unverified and possibly unverifiable speculatiors about the
development of societies when energy and time could much more
profitably be devoted to a study of actually observable societies
as they are at present.

Secondly there is a philosophically much more interesting
resistance to taking notice of the past, a resistance whose logic
may not be wholly clear to all who are affected by it.   This
resistance is curiously analogous to the familiar one in physics
against " action at a distance ".   Just as one is intuitively
repelled by any causation between bodies that is not a case of
" push ", so one is repelled by the idea, which *seems* implicit in
" historical explanation ", that what goes on here and now, what
men living now are doing, requires for its understanding the
specification of events or situations long past. It is even more
repellent than action at a distance ; for in some sense it is true
to say that " the past does not exist " and the present does.
How could, one might say darkly, the unreal and non-existent
shackle the real and existing ?   When anthropologists say, as
sometimes they do, that " there are no survivals ", meaning that
present goings-on must be explained in terms of their present role
and not genetically, what they say seems equivalent to saying
that the past has no effects, or no significant ones.   (They may
be prepared to explain what is merely " cultural " genetically,
ipso facto implying that it is a mere historical accident, whilst
refusing such explanations for " structural " facts.   Roughly
speaking, the fact that we express respect by taking off our hats
is cultural, but the fact that certain groups receive expressions
of respect is structural.)   But if the past has no effects, what has ?
Is not the past the *only* thing that could have effects ?   The
argument is in a sense valid—only present forces can *operate* at
present.   In other senses it is invalid—and the most important
of these is that the past may be and often is a necessary source of
evidence about what forces are operating at present.   Roughly :
the dynamics of a situation could never be worked out from a
snapshot alone.

" The explanation of the present must be sought in the
present." This argument is never stated as openly as this but
it does support, and is in turn supported by " Functionalist "
theory which maintains that a society is a " system in equilibrium

with mutually determining parts ", with the crucial implication
that explanation must be sought in the inter-relation of those
parts rather than in anything else.   If this doctrine were stated
openly its difficulties would promptly appear, if for no other
reason than that the interdict on the past on such grounds would
strike on everything except the literal, immediate and shifting
present—a basis too tenuous for even the most " functionalist "
and equilibrium-minded anthropologist.   There is a wealth of
philosophical perplexities here, probably related to Zeno's.   For
" The past can never be a cause " and " The past is the only thing
that can be a cause " both have some appeal.   Or in other words
anything truly *past* is too infinitely distant, separated by the
chasm of the unreality of the past, to affect the present, even if
it is only past by a second ; a miss is as good as an epoch.   At the
same time the present is too thinly insubstantial, too devoid of
weight, to *cause*.   Anyway, can one think of a series of self-
causing instants ?   Moreover, no instant has any extension, and
however many of them we add together we get no continuum ;
zero multiplied by any number is still zero.   Thus the apparent
vistas of time are illusory ;   there is but one eternal instant.
This is not the place to pursue these puzzles, but it is worth while
indicating them as a kind of last backcloth to the more concrete
discussions about change and the past in the anthropological
context.

A similar dispute occurs within psycho-analytic theory, be-
tween the view that the events of infancy are literally of aetio-
logical importance and the view that only the psyche " as it is
now functioning " matters.   Analogously, some anthropologists
are tempted to say that " remembered history (alone) matters ".
I have heard this kind of view expounded by an anthropologist
and attributed with grateful recognition to Malinowski.   But
Malinowski is dead.   How long can a dead Functionalist be
influential without a pragmatic contradiction ?

The reason why the extremist argument operates, as I suspect
it does, at the back of anthropologists' minds, is that it is mixed
up with other considerations, some of them very cogent, and is a
kind of limiting, metaphysical variant of them.   These various
considerations overlap and tend not to be distinguished, but I
shall attempt to sort them out.

One of them is hostility to what one may call the fallacy of
origins.   Suppose a historical series of situations ABBBBB, or
even a series of ABCBCBCBC ; a person is the victim of this
fallacy if he believes that to discover and indicate the occurrence
of the initial situation A is all that is required, and all that

could be provided, for the explanation of the present situation
(B or BCB . . .).   There are indeed occasions in daily life and in
historiography when this type of explanation is held sufficient.
If I come into my room after an absence and find a strange object
on the floor, the fact that the cat brought it in is sufficient.
But the real reason why this specification of the initial episode
in the history of the object on the floor of my room is sufficient
is that the principle underlying its subsequent staying there is
so obvious that no one would dream of bothering to make it
explicit : barring earthquakes, hurricanes, etc., inanimate objects
of certain weight do not move away from their positions on level
floors.   The situation may be analogous, sometimes, in historio-
graphy, though here the anthropologist rightly has his doubts, for
the life of nations is not a level floor and institutions, culture
traits, etc., are not inanimate objects.   With some anthropolo-
gists, a distrust of historical explanations is combined with a
distrust of (some at least) historians, who are suspected of using as
obvious or trivial principles of explanation principles which may
be either not trivial or not clear or not true.   Historians are
particularly suspect of " ethnocentricity ", of facile projection
on to others of temporarily, geographically or socially local
motivations, outlooks, assumptions, etc.   Anthropologists con-
sider their own initiation or *rite de passage,* field-work in an alien
society, to be an effective cure for such projections, though
perhaps some of them then tend to project the assumptions of
the culture they studied.

Slightly different and more sophisticated than the fallacy of
origins is what I am tempted to call the sociological versions of
the genetic fallacy.   One tends to use this expression in connexion
with beliefs, to designate the mistaken attitude of someone who
thinks that the history of the acceptance or holding of a belief
is sufficient evidence for the assessment of its truth and merit.
The sociological version of this is the assumption that the speci-
fication of the series leading to the present explains the present.
This is a little superior to the fallacy of origins in that the whole
series and not merely a privileged episode in it is required, but
it is still insufficient in that a sequence is not an explanation ;
one wants the generalisations connecting the items in it or the
glue between the links.   Anthropologists certainly do.   Hitherto,
they tended to concern themselves with the glue between links
of a chain of the form BBBBB—in other words with the mechanics
of the self-maintenance of stable systems, as Leach points out.
Leach himself is in the more specific parts of his book pre-occupied
with the mechanics of a series of the form BCBCBCBC.   But

anthropological theory has been so much preoccupied with socie-
ties which seemed to be of the BBBB kind that he thinks that
those which are not are epistemologically intractable ; and his
worry is aggravated by the realisation that to some extent all
societies are *not*-BBBB.

Ironically anthropologists had initially come to be pre-
occupied with stable societies mainly because they were interested
in primitive ones not *qua* stable, but *qua* evidence for genetic
explanations.   Nowadays, genetic explanations are out of favour;
" Structural " explanations are the thing.   It would take too
long, and be too difficult, to attempt to analyse that notion here.

The appeal of genetic explanations is curiously powerful.   It is
extremely hard to convince people not acquainted with anthropo-
logy that in studying the such-and-such people, one is *not*
preoccupied with " where they came from ".   The genetic
approach, for instance, probably also underlies many epistemo-
logical fallacies, operating through the assumption that specific
source must be found for any item of knowledge.   One is tempted
to speculate about the genetic outlook itself being a survival from
the social forms in which to place a man genealogically was to
place him in other ways too, or from those cosmogonies which
are content to explain everything by specifying the initial
episode.

The objection to explanation by origins could be formulated
so as to bring out an interesting analogy between physical
cosmology and social studies, by asserting as a principle that
there are no privileged moments ;[1] that a symmetry operates such
that no time-place position requires more or less explanation.
Though the universe is not the same all over, the same funda-
mental laws operate all over.   The principle is explicitly denied
by many religions—a difference which is probably a crucial and
defining difference between the two kinds of thought-systems.
In a science such as physical cosmology which must make such
extensive use of extrapolation, this principle is constantly in use.
The interesting thing about the social sciences, in which the ratio
of extrapolation to independent confirmation is quite different,
is that the assumption operates though it is held to be false.
There are privileged moments in human affairs if there are, as is
presumably the case, " decisive moments of history ", *i.e.* single
episodes with vast effects, where the episodes themselves were
quite unpredictable—not because of an inherent indeterminism
but because, as with the fall of a penny, too many factors too
minute to be considered came into play, or because they entered

[1] *Cf.* Levi-Strauss, *Les Structures Elementaires de la Parenté*, p. 27.

the system from outside. Who could foresee the shape of Cleo-
patra's nose ? The occurrence of Cleopatra's Noses isn't denied,
but they simply elude explanation in the social sciences as such.
In the natural sciences, *e.g.* in biological genetics, Cleopatra's
Noses such as mutations can be dealt with statistically ; but
that again is impossible in the social sciences for our runs and
populations are not large enough, and our focus of interest too
specific.

Explanations by origins violate the principle of no privileged
moments by implying that the origin alone explains, and explains
sufficiently. Functionalist anthropologists when rightly rejecting
this have gone too far and insisted that origins never explain at
all. Also, when rightly insisting that stability as well as change
requires explanation, they have tended to equate theories of
society with theories of the maintenance of stability—so much
so that the paradigm of explanation of a social feature is held to
be an explication of how it contributes to the maintenance of
the *status quo*.

The late Professor Radcliffe-Brown said that the trouble with
conjectural history (speculations about the development of
illiterate peoples) was not that it was history, but that it was
conjectural. The real situation seems to me to be that the trouble
with it is neither that it is history, nor that it is conjectural,
but that, *by themselves*, past events are no explanation. One
might say : the trouble with hen-and-egg questions is not that
we don't know but that we don't care. It would not advance
our understanding of hens or eggs. But "functional" hypo-
theses can *also* be conjectural ; the real contrast is not really
between conjectures about the past and tested truths about the
present, but between sociological hypotheses tested in the present
and the past, and genetic accounts that pretend to greater logical
force than they can possess, or to being the only possible explana-
tion. That a genetic reconstruction is purely speculative is an
aggravation of the offence, but not its essence.

Thus distrust of history has some validity but calls for
some cautions. For one thing, though historical facts alone do
not constitute explanations, they are evidence for explanations
of another kind, and moreover this other kind finds as rich a field
in the past as in the present. The importance of documentation
for purposes of reconstructing the past may be exaggerated, and
the line between ' conjectural " and " genuine " history drawn
too sharply, for even when documents are available they are not
evidence without interpretation, and other things may throw
light on past social structures. Moreover, no virtue should be

made of other people's necessity—the documented past should not be ignored where documents are available, on the grounds that there are none on the Trobriand islands.

The physiological analogy springs to the minds of anthropologists easily ; indeed, the investigation of the functioning of an organ is quite independent of the biological history of the species. But the physiologist can ignore phylogenesis only because the life-cycles of succeeding generations really are *cycles*, they really do not differ sufficiently.   He cannot ignore the life history of the single organism, even when considering, for instance, the heart, whose rhythmic motion might *prima facie* also be held to constitute a mechanism whose understanding does not require considerations of the past.   The physiologist does consider temporal processes over a relatively small span, and ignores those of a large span because he can rely on them to be wholly repetitive, with differences insignificant for his purposes.   The anthropologist cannot *rely* on anything of the kind, whatever the span may be.   The special attention given to stable societies without records and the kind of theory this tended to produce have made the physiological analogy seem more plausible than it is, and have, rightly or wrongly, led to anthropological theories appearing to carry the analogy between societies and organisms further than is justified.   Even the human body has the functionless appendix.   Anthropologists should be willing to concede that some societies may sometimes have appendices too, even if they are rightly incensed against the magpie theory of culture which makes societies consist of nothing but appendices.   By ' Magpie theory of culture ' I mean the doctrine or method which implies that a society can be explained by specifying when and whence each item in its make-up came, as historians of ideas sometimes seem to think that they have accounted for a climate of opinion by locating the books and authors in which its constituents first appeared.

It is instructive to compare the paradigm of explanation as conceived by the historian, and by the anthropologist.   A historian is, I imagine, overjoyed to discover a contemporary or later institution in a more rudimentary form at an earlier date.   To have a fairly continuous series of such forms beginning with something that is almost zero, so to speak, is to have a good account of " the development " of the institution and hence an explanation of it.   The anthropologist seldom gets an opportunity for achieving something of this kind, but if he did he would not be satisfied with it.   His instinctive reaction is, I think, to reflect that " in its rudimentary form " the institution probably

played quite a different role from what it does in its later, " fully-developed " stage, and that the tie-up of the two stages, apart from being quite possibly based on nominal or superficial similarities does not explain the function and functioning of the institution at either of the two times. He may add that the historian is tacitly employing a premiss that is both nebulous and questionable, namely that it is enough for a precedent to exist for it to be repeated and improved upon, which is what tracing of developments amounts to. This will not do, for paradoxically there are both more precedents than repetitions, and more activities than precedents. The anthropologist may feel, with some justification, that the historian takes for granted and lucid just that which is most problematic—the glue between segments of a temporal sequence, above all between similar segments, between stages of stable society.

The historian may reply, also with some justification, that the Functionalist anthropologist anxious to explain the present by the present has, to take the extreme example, no way of including what may be called explanations by *trauma*. The documented histories of literate peoples provide examples of exceptional and dramatic events which, as we say, " left their mark ". There is no reason for supposing that none such occurred in the past of peoples whose histories cannot be reliably reconstructed. But the marks such events have left cannot be adequately accounted for without reference to those events. Now the point which is brought home so strikingly by these traumatic events in fact applies equally to all events, traumatic or not, which are, at any remove, amongst the efficient causes of present situations.

In this opposition, it seems to me clear that the historian may err through inadequate interest in the nexus between succeeding events, or willingness to accept superficial accounts of it, an error not likely to be committed by the anthropologist ; *his* interest in the " social structure " is in effect an interest in what kind of events resulting from the interplay of which factors do and can follow each other in a given social environment. But having formulated the problem more correctly, he may err through neglecting the past as a storehouse of evidence about it. Leach seems to me to be right in pointing to this error, but wrong in diagnosing it. This error does not depend on the fact that societies change whilst concepts do not. It would continue to be an error even if no society did change, though it would not then do much practical harm, and even although in fact concepts do not change, not being the kind of thing that either changes or does not. (A concept that has changed is not the same concept.

There is a sense in which " concepts change ", but it is not relevant here.)

It must be stressed that many anthropologists do not in fact disregard the past when they have relevant evidence ; nor does Leach say that they do. He merely says that when they do consider it, this does not fit their concepts, and they do not know what to do with it. Even at the level of meta-theory, of assessing their own activities the situation is not wholly clear cut, for one very distinguished British social anthropologist, Professor Evans-Pritchard, recently classed anthropology with history. But it is not yet clear how such a slogan would fit into the main tradition of British anthropologists' way of looking at their subject.

Anthropologists' rejection of explanation by origins reminds one of Professor Popper's critique of historism (not —icism). At the same time, however, their attitude to stable unchanging societies is different. They are not hostile to them, nor do they exaggerate the extent to which " tribalism " is free from moral and social doubt, status-insecurity, etc. Direct contact with tribes has refuted not merely Hobbes, but equally the " safe, cosy social womb " theory. Past idylls, even when rejected, are suspect things. If the foetus could speak, the doctrine of birth trauma might have to be revised, and what Adam's views were on the garden of Eden is not well established.

These then are the considerations which may lead some anthropologists to reject history, and I hope to indicate that either they lack cogency or when properly understood do not imply a disregard for history. Thus Leach's final conclusion and recommendation are right, but he need not have undertaken his excursus into Idealism in order to demonstrate it. His book may also give one an exaggerated impression of the extent to which contemporary anthropologists are in need of this lesson.

One of the main objections to failing to consider historical evidence is that concepts with which social anthropologists operate are, most of them, what philosophers call " dispositional " ones. A statement concerning, for instance, the real functions of a chief or of a ritual in a given society simply cannot be verified without taking into consideration what would happen in situations other than the one which holds at present, and one important source of evidence about such alternative situations is the past. Professor Ginsberg has said that the concept of " social class " denotes not a real group but a *potential* one. One should notice that this is true of very many sociological concepts.

Another consideration is that in studying the pattern of cause and effect in a given society, anthropologists are not so much

interested in the initial conditions of a sequence, as in the general-
isation specifying the relation between the condition and the con-
sequent states of affairs. This is so, and should be so ; though
in order to use this as a satisfactory analysis of anthropological
explanation, one would have to go further and take into account
the fact that the generalisation itself is something that needs
explaining, as in other sciences though in a manner which may be
peculiar. For one thing, the generalisation itself may describe
a regular connexion which is a state of affairs characteristic of a
society, and this fact itself be the consequent in a more general
sequence ; but in order to know this we must also know the ante-
cedent, and thus *historical* evidence becomes absolutely necessary.

I do not wish to give the impression that I criticized Leach's
argument as an example of ordinary error. If it is erroneous, it is
so in a way in which interesting philosophical doctrines are. On
the contrary, it is perhaps the most lucid statement of a certain
kind of Idealism that I know, and teachers of philosophy could
profitably use a selection of his statements as a means of explain-
ing to their students what such Idealism is about. They
certainly could do no better than use Leach's book if they wish to
illustrate how philosophical problems can spontaneously grow
out of first-order work in an actual science. Moreover, the criti-
cized argument is used to point a moral which is in fact a good
one ; and philosophical error analogous to his own is as present,
if not as clearly stated, in the views he criticises as in his own.

London School of Economics

V.—SCIENCE AND MORALITY

By K. Kolenda

The question of the cognitive status of ethical utterances cannot be settled without a discussion of the nature of cognitive utterance in general. For this reason the similarities and differences between statements expressing scientific propositions and statements expressing moral judgements need to be explored. This, in part, is the concern of my paper. I shall conclude that the similarities are substantial enough to justify us in regarding moral utterances as cognitive. At the same time, the concluding section of the paper will show that, in virtue of possessing some features of its own, moral inquiry cannot be fully assimilated to the procedures of natural sciences.

I

Propositions of natural sciences are generally held to be examples of cognitive utterance *par excellence*. Natural phenomena are properly understood when they are explained scientifically. But what does it mean to see natural phenomena from a scientific point of view ? Confronted with a question : " What is the phenomenon X ? " a man has a problem of identifying or describing X properly. This calls for invoking some law-like framework antecedently known to be established. The answer can be given only if there is a law or a set of laws under which the phenomenon can be subsumed. Thus the concept of a law is a key notion of science. To have a scientific view of phenomena is to view them as manifesting certain laws or regular patterns, expressed by means of general conceptual frameworks. Undoubtedly scientists distinguish between laws and principles, and also between various kinds of law. But our discussion does not require a distinction between various types of conceptual framework. All we need is to observe that without some general frameworks, in terms of which encountered phenomena may be explained, no science can exist.

The connection between conceptual frameworks and cognitive utterances should now become apparent. In relating the observed phenomenon to the features which a conceptual framework formulates, we obtain some new and relevant information about this phenomenon. For example, Snell's law helps us to discover that in the phenomenon of refraction transparent media

exhibit constants, and that these constants can be calculated from
the formula of the law and the measurements of angles of incid-
ence and refraction.   Since our awareness of this law enables us to
formulate propositions which convey some new and relevant
information, the statements which are made on the basis of this
law can be rightly regarded as cognitive utterances.   Further-
more, we can affirm that a conceptual framework itself deserves to
be called cognitive if it helps us to discover or to become aware of
some important or interesting features of experience.   In general,
we can say that to be engaged in a scientific inquiry is to try to
find out something.   The problem of the scientist is to " dispose "
of a phenomenon rationally, that is, to locate it on a presupposed
valid conceptual scheme.

   When we turn to moral inquiry, we notice an analogous
situation.   Confronted with a question :   " What am I to do in a
situation Y ? ", a man has to determine the nature of his problem
in terms of familiar and relevant moral rules.   This calls for the
antecedent recognition of the validity of such rules.   Although a
systematic moral inquiry may differentiate between principles and
rules, this distinction is not necessary for our purpose.   All we
need to do is to consider the narrowest conceptual framework in
terms of which moral agents describe certain actions.   The term
" moral rule " is here regarded as the narrowest conceptual
framework of moral inquiry.   This term has become technical
enough in contemporary philosophical discussion to be free of
casuistic connotation.   There is widespread agreement that most
or perhaps all moral rules are defeasible, but that the defeasibility
of a moral rule in some cases, does not weaken its general regula-
tive validity.[1]

   With this in mind, let us return to our analysis of moral
inquiry.   Although a moral reflection aims at a particular and
specific decision, this decision is withheld until the agent answers
for himself the question :   " What moral rules, if any, are relevant
to my present situation ? "   Thus his deliberation is an articula-
tion of the possible course of action in terms of a relevant moral
framework.   The same is true for a moral critic.   When he
judges some actions as moral or immoral, he judges them in the
light of some presupposed moral rules.   Without invoking a
general moral standard, neither moral deliberation nor moral
criticism is possible.   Consequently, we may affirm that the aim
of moral reflection is also to *find out something.*   Such reflection
intends to appraise an action or actions in terms of some moral
conceptual frameworks.   Moral rules provide these frameworks.

[1] *Cf.* L. G. Miller, in " Rules and Exceptions ", *Ethics* (July, 1956).

The crucial question, however, is whether the appraisal of actions in terms of moral rules is a way of providing information about these actions. If this is the case, then moral utterances deserve to be regarded as cognitive ; if not, cognitive status must be denied to them.

I believe that the ascription of moral character to some actions *is* a way of providing new information about them. To subsume an action under a moral rule is to point to some facts or relationships which would remain undisclosed without such a subsumption. For example, our awareness of the rule "Stealing is wrong", *makes it possible* for us to identify some actions as instances of stealing. To label an act as "stealing" is to provide information about this act. Such labelling indicates that the appropriation of an object is unauthorized and violates a recognized property right. This violation in turn, entails destruction of trust and points to possible occurrence of harm, retaliation, etc. In this way, the rule helps to discover that some forms of taking possession of someone's property have special features.

There can be no doubt that in daily life the reference to actions which are seen in the light of some moral rules, has informative value. When, for example, I am criticized for not returning a borrowed book, I am not only informed that my possession of the book has a special feature : it is available to me on some conditions—I am to take good care of it, return it at a set date, etc. I am also informed of the fact that my having the book on these special conditions keeps producing effects independently of the decisions which I might make in respect to the book. These effects may be many : the lender will regard the book as his possession, he may list it as one of the items in his library, he may sell it, or promise it to someone else, he may plan to read it as soon as it is returned, etc. Of course, all of this is not explicitly contained in the statement, "You ought to return the borrowed book". But in practice, whenever and wherever people acknowledge and respect some moral rules, they do make inferences about the expected course of events from the use of moral language. Thus, when I am known to respect promises, a friend who had lent me a book may count on my returning it on a promised date and may promise the book to someone else prior to my returning it. In a community which takes seriously some rules of behaviour, human actions and their effects have a high degree of predictability and moral judgements have informative value. Furthermore, since moral rules provide general concepts in terms of which such judgements can be made, we are justified in regarding moral rules themselves as *cognitive* conceptual frameworks.

II

The possibility of disclosing some features of experience by means of a conceptual framework constitutes what might be called its descriptive or theoretical aspect. But it should be noted that all conceptual frameworks have also a prescriptive or practical aspect. The word " all " in the last sentence is important. Recent philosophical discussion tended to view description as the primary function of factual scientific statements, and prescription as the main function of ethical judgements. This is a mistake. Just as ethical statements have a descriptive side, so factual statements have a prescriptive aspect. A little reflection will show that this is the case.

The function of a conceptual framework is to help us discover some facts or relationships. But not all existing facts and relationships are of equal interest to us. Some of them may be of no interest at all. For instance, in admitting students to a university we are not concerned about determining the relationship between the colour of their eyes and their intelligence. We may have good reasons for this, but the fact remains that we do ignore some features of experience. Our investigation of facts and relationships is always selective. Consequently, we need not be interested in possible conceptual frameworks whose function would be to disclose relationships which are of no concern to us. This indicates that the prescriptive aspect of a conceptual framework is a hypothetical imperative. If the facts or relationships disclosed by the given conceptual framework are in some ways significant, then this framework becomes relevant. This applies to science in general. A scientific framework is something which *may* be applied to nature, if men choose to do so. A man may or may not look at some phenomena scientifically, or he may choose his own scientific framework. For example, a farmer may show no interest in relating his planting and harvesting to the conditions of soil and weather. Or, instead of studying the chemistry of the soil and the nutritional needs of his crop, he may do his planting and harvesting by the signs of astrology.

The same can be said in respect to morality. Moral rules *may* be applied to behaviour, but again, only if men choose to apply them. A man may or may not see his and others' actions from a moral point of view. He may consider human behaviour as merely biological or physiological process, and regard it as a play of impersonal, amoral forces. But he may also look at human actions as performed in accordance with or contrary to some moral precepts. Thus, he may view the appropriation of someone's

property simply as transfer of objects, or, *in addition* to this, he may recognize it as stealing.

Furthermore, scientific knowledge and its application may significantly diverge. We may know what is the proper way of determining the specific gravity of some material, and still disregard this way, falling back on a rule of thumb or a mere guess. This is likewise true of morality. Although the aim of moral deliberation is to provide moral understanding, this, in itself, does not guarantee moral action. We may know what we ought to do, and still fail to do it. Perhaps the most difficult part of moral life is in the effort to attain a harmony of moral understanding and moral practice. It is for this reason that Aristotle and others underscored the importance of habituation of the will.

If there is a gulf between knowledge and its application, it is just as great in science as in morality. But is there such a gulf? If we realize that the descriptive and the prescriptive aspects are found both in science and in morality, we shall not regard science as a primarily theoretical enterprise and morality as a primarily practical concern. The theoretical and the practical aspects *together* constitute an inquiry. Knowledge, as the fruit of inquiry, satisfies both the theoretical and the practical needs. To have knowledge is to see the encountered phenomena in the light of some fruitful conceptual frameworks. We may remain ignorant of some features of experience because we are unaware of the conceptual frameworks which, if applied, could disclose those features to us. In this sense, knowledge is a value among other values, and has a normative aspect. Of those rational animals who, contrary to Aristotle, do not by nature desire to know, we still could affirm that they ought to do so.

III

The use of conceptual frameworks in any cognitive enterprise stems from the recognition of their prescriptive aspect, that is, from the realization that reliance on such frameworks can lead to significant information in some area of experience. A philosophy of science which fails to recognize this prescriptive aspect of scientific laws has considerable difficulty in giving a satisfactory account of their logical status. In this part of the paper I shall examine a recent discussion of the logical status of scientific laws, in order to show how an unsatisfactory treatment of this question may throw a sceptical cast over the whole enterprise of science. At the same time, this examination will be instructive in another way : it will disclose the path which leads to the denial of the cognitive status of ethical statements.

In his recent book, *The Philosophy of Science*, Stephen Toulmin examines the view, attributed to Moritz Schlick and F. P. Ramsey, that laws of nature may be looked upon as prescriptions or rules for formulation of empirical propositions. Although Toulmin sees some merit in this view, he is unhappy with the way in which it is put. He finds that this way of speaking employs " unduly imperatival " [1] language, and, what is more serious, " snaps the link between laws of nature and the world ".[2] He believes that to look at scientific laws as prescriptions is misleading because this creates the impression that the laws do not really have anything to do with nature, but only with the conduct of scientists. Surely, Toulmin objects, scientists are not talking about themselves when they are formulating the laws of nature.

But it is doubtful that Schlick and Ramsey would admit the conclusions which Toulmin drew from their proposals. From what Schlick has said of scientific laws in other contexts, we can be reasonably sure that he would reject altogether Toulmin's conclusions. In an article entitled " Are Laws of Nature Conventions ? " Schlick makes clear that the statements of laws of nature refer to some objectively existing states of affairs. He denies, for instance, that by applying to nature various systems of geometry we are obtaining different physical laws. He adds : " If at all possible, we would like to understand by a ' law of nature ' something which remains invariant in respect to every arbitrary mode of expression. And this is possible. . . . That which constitutes the real content of a law of nature, is expressed through the circumstance that to definite grammatical rules (*e.g.* of geometry) belong quite definite propositions as *true* descriptions of reality, and this circumstance is wholly invariant in respect to every arbitrariness in symbolization." [3]

In fairness to Schlick, his view of the prescriptive character of scientific laws ought to be differently construed. To avoid Toulmin's conclusion, one must specify that *the prescriptiveness of a law cannot be separated from the concrete context of inquiry within which it is formulated.* When this qualification is added, it still remains true that the acceptance of the validity of a law commits the scientist to some specific behaviour in respect to the phenomena explained by this law. But what is also true is that in thus looking at phenomena in the light of this law the scientist will perceive among them an objective and independently existing relationship. This is what Snell had discovered when he succeeded in formulating the law of refraction. The use of a

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[1] *The Philosophy of Science*, p. 101.   [2] *Ibid.* p. 102.
[3] *Gesammelte Aufsätze*, Wien, 1938, p. 319 (my translation).

theoretical framework expressed in a law brings additional order into the scientist's comprehension of the relations among the observed phenomena. In this way, the law provides a new understanding of the phenomena to which it applies.

In general, we should not isolate the prescriptive aspect of scientific laws or of other conceptual frameworks from the special kind of contexts in which they are meant to be applied. It is clear that Schlick construed laws of nature as specifying conceptual directions for physicists *as they are engaged in doing physics*. And " doing physics " means disclosing the relation of one's formulations, or one's way of looking at phenomena, to those phenomena themselves. The new way of looking at things is not a discovery about physicists but a discovery that some phenomena can be better explained by means of this new way. Only if we leave out this important qualification can we be led to the view that scientific laws do nothing but direct the scientists' behaviour. Conversely, if we keep this qualification in mind, even the seemingly puzzling pronouncements of some scientists may sound quite plausible. When Einstein says that the axiomatic basis of theoretical physics is a work of imagination and " cannot be abstracted from experience but must be freely invented ",[1] we need not be unduly disturbed about this claim. We need not object, as Toulmin does, that " this is not work for untutored imagination. It may be an art, but it is one whose exercise requires a stiff training."[2] Surely Einstein's remark cannot be construed as applicable to just *anybody*. Only competent physicists familiar with the phenomena under investigation could conceivably constitute a class of persons to whom Einstein intended to apply the remark.

I suggest that Schlick's view of the logical status of scientific laws is not as faulty as Toulmin seems to think. On the contrary, it takes into account the importance of the contribution made by men to their respective fields of inquiry. Toulmin is aware of this when he says that " the laws themselves do not do anything : it is we who do things with them, and there are several different kinds of things we can do with their help ".[3] And again : " It is not that the physicist has a mysterious predilection for some theoretical moulds, into which he thrusts all the experimental results he meets, nor is it a deep necessity of experience that he should handle these results in the way he does. His part is no more than that played by anyone who introduces a language, symbolism, method of representation, or system of signs."[4]

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[1] Cited by Toulmin in *op. cit.* p. 43.   [2] *Ibid.* p. 43.
[3] *Ibid.* p. 89.   [4] *Ibid.* p. 128.

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It is important to remember that this part needs to be played.
But what is even more important is to realize that the game of
science is not being played in a vacuum, and that the scientific
formulae are applicable to real phenomena.   Einstein never lost
sight of this, even when he spoke of the importance of imagina-
tion.   " Although the conceptual systems are logically entirely
arbitrary, they are bound by the aim to permit the most nearly
certain (intuitive) and complete co-ordination with the totality of
sense-experience. . . . . A correct proposition borrows its ' truth '
from the truth-content of the system to which it belongs." [1]

If we realize that science involves both theoretical and practical
aspects, we may avoid another misleading dichotomy.   One often
hears that science is not interested in the causes of phenomena,
but only in the laws of nature.   According to this view, supported
also by Toulmin,[2] causes are explanations of particular pheno-
mena, and this is a task of applying a law to an instance of it, a
purely practical matter.   Therefore, the task of a scientist is
merely to establish a law, and leave the application to others.

This view is interesting because it raises the question of the
whole point or purpose of the scientific enterprise.   Why are
scientists interested in laws ?   To this I would suggest an answer :
Because men are interested in causes.   Because men want to
understand particular phenomena.   Consequently, unless scienti-
fic laws explain particular phenomena and make them intelligible,
they are not laws of anything.   Scientific inquiry is kept alive by
the presence of phenomena for which an explanation in terms of
laws is not yet available.   Our inability to explain these pheno-
mena prompts us to seek for them a place in a framework of laws.
When this is achieved, a new cognitive value is brought into
existence.   This value consists in our ability to account for the
phenomena which antecedently were not understood.   As
Toulmin correctly observed :   " The problem of applying the
theoretical calculus remains in physics the central problem, for a
science is nothing if its laws are never used to explain or predict
anything ".[3]   The purpose of scientific inquiry still remains :
rerum cognoscere causas.

I have dwelt at some length on the problem of the logical status
of scientific laws because I wanted to point out the close relation
of the prescriptive aspect of scientific laws to the context within
which they yield descriptive information.   Toulmin's objection
to Schlick's view is particularly interesting in this connection.
What Toulmin feared—and it is easy to sympathize with him in

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[1] *The Philosophy of Albert Einstein*, P. A. Schilpp, ed., p. 13.
[2] *Op. cit.* pp. 119 ff.   [3] *Ibid.* p. 108.

this apprehension—was the possibility of separating the pre-scriptive or regulative aspect of laws from the subject matter which those laws are to explain. This is the reason why Toulmin was so concerned about the misleading language in which Schlick chose to express his view. The wording seems to produce most unfortunate results for our knowledge of the world : it snaps the link between science and nature. But this means that even natural sciences are " non-cognitivist " !

The way out of this predicament, as I tried to indicate, is to deny that the prescriptive aspect of laws can be separated from the specific contexts within which these laws function descrip-tively. To steer clear of this mistake is both to preserve the cognitive character of science and to recognize the contribution made by human thought to the understanding of nature. But this mistake is instructive also in another way. It discloses the way in which the so-called " non-cognitivist " theories of ethics come into being. These theories are the consequence of dis-regarding the concrete context within which the prescriptive function of moral conceptual frameworks is to express itself. As the unduly " imperatival " view of scientific laws shifts the attention from described phenomena to the scientist who describes them, so the unduly " imperatival " view of moral rules shifts the attention from the actions to the agent. Consequently, instead of saying something about the character of the action, ethical statements are construed as merely revealing something about the agent—his feelings, wishes, or attitudes.

As we have seen in the first part of this paper, the application of moral rules to some actions does yield further information about the character of these actions. A moral judgement does not lift the judged actions out of the concrete context within which these actions occur. On the contrary, it is explicitly meant to apply to this context. Actions are events brought about by men in a natural world. For this reason, moral utterances, while they interpret actions in the light of some moral concerns, at the same time refer to the factual features of these actions, including their possible or likely consequences. Thus, in relating the actions and their consequences to a relevant moral framework, moral utterances have an informative function. The relevance of this moral framework is, as noted before, a hypothetical imperative. If the prescriptive aspect of the moral framework is not recog-nized, that is, if there is no concern for what the application of this framework may disclose, then, of course, it cannot be put to a cognitive use. This framework is a conceptual articulation of the moral significance ascribed to some types of action. In a

community which should discern no moral relevance in some of
the actions, moral discourse could not take place.   But where
such relevance is recognized, moral inquiry and discourse are an
important part of rational life.

IV

In the first part of this paper I defended the view that ethical
statements, like scientific statements, deserve to be regarded as
cognitive utterances.   In the second part I indicated that all
inquiry, including science, has a prescriptive aspect.   An analysis
of the logical status of scientific laws led me to observe, in the
third section of the paper, that it is a mistake to separate the
prescriptive aspect from the special context within which it is
relevant, and that this mistake is responsible for the emergence of
" non-cognitivist " theories of ethics.   The foregoing considera-
tions pointed up the similarities between science and morality,
thus giving support to the view that both enterprises ought to be
recognized as cognitive.   In this final section of the paper I shall
introduce another way of comparing these two fields of inquiry.
Although this comparison will bring out a further point of
similarity between science and morality, it will also help us see
the special features of the latter.

Both within science and within morality it is possible to
distinguish between what I would call a *routine* and a *frontier*
aspect.   First, what is the routine aspect of science ?   After a
law has been established, the phenomena for which it was found
to hold, fall under it as a matter of course.   For instance, if it
has been established that Snell's law holds for most non-crystal-
line materials of uniform density under normal conditions, then
the scientific method of determining the given angle of refraction
follows deductively from the formula.   The strict formulation of
a law determines what are to be the phenomena which verify it.
One does not get more out of a law than one puts into it.   If for
such and such phenomena the law has been found to hold, then
only these phenomena can qualify as verifying the law.   Hence
the application of laws to phenomena is a matter of simple
subsumption.

By the frontier aspect of science I understand these areas where
actual scientific advance is being made or needs to be made.   In
this area the active scientific thinking arises out of perplexity.
Scientific perplexity does not exist when phenomena obviously
fall under well-known and accepted laws.   But when a science
is being developed, when it deals with phenomena for which
scientific formulation is not yet available, there is a need for

genuinely creative thinking. Such thinking goes beyond the routine aspect, because mere application of established laws and procedures does not yield the desired understanding.

If the question is raised as to which of these two aspects is more characteristic of science as active inquiry, the answer is clear. What is known is no longer a problem. The well-established findings of science, no matter how important and useful, are of little interest to a man whose problem is not the known but the unknown. Without at any time detracting from the value of the consolidated scientific information, we may nevertheless say that, as an inquiring, knowledge-pursuing being, the scientist will not waste his time on what has been already discovered and explained.

In turning to morality, we find that it also exhibits both a routine and a frontier aspect. In guiding our daily activity we constantly subsume some actions under familiar rules of behaviour. As a matter of course we keep our appointments, return borrowed books, help a blind man across the street, give a push even to a total stranger in a stalled car. And this is done without a further thought. Of course, whenever our action is challenged, we may cite a rule from which the propriety of the action can be deduced. But in actual practice we are hardly ever asked to justify such familiar and generally accepted proprieties of action.

The frontier area of moral reflection is characterized by the need to evaluate those actions which are not obviously covered by moral rules. Like scientific inquiry, moral deliberation arises out of perplexity, which initiates a search for a possible solution. This solution must not be arbitrary. It must have general validity. For the point of deliberation is not just to *settle* the particular problem confronting us. It is to settle it in such a way as to provide us with a general understanding of the character of our decision. What we are after is a general orientation, an insight, which could be used as a directive not only for this particular situation, but also for possible future situations of a similar kind. We want to find out whether the characteristics of the action to be performed or criticized can be fitted into a framework of reorganized and re-appraised moral rules. In the same way, the scientist investigating an unexplained phenomenon hopes to establish the presence of some features which would provide a general understanding of the nature of this phenomenon.

Again, if we should ask, which aspect is more characteristic of morality as active inquiry, the answer would not be difficult. Here also we need not underrate the importance and the value of actions which stem from a habitual response to familiar moral

proprieties. But it seems that all genuine moral deliberation has a frontier character. Consequently, we get a better picture of moral inquiry when we regard it as analogous not to the routine but to the frontier aspect of science. Only when we remain within the routine aspect of science can we provide precisely formulated laws from which scientific descriptions of phenomena can be logically deduced. By the same token, if we remain within the routine sphere of morality we can point to the logically deductive character of actions in respect to moral rules. But when we reach the frontier areas of either inquiry, automatic subsumption and deduction is no longer possible. We recognize in moral reflection what Einstein recognized in science : the frontier aspect of thought calls also for imagination, intuition, insight.

Since moral reflection almost always has a frontier character, we should not expect that the exact scope of moral rules can be formulated in advance. While science as an institution is explicitly concerned with *providing* exact scientific descriptions and classifications of natural phenomena, moral inquiry has no such single institution nor procedure devoted to exact and definite formulation and establishment of moral standards. Perhaps the closest we can come to explicit formulation of moral standards is in the translation of moral convictions into a legal code. But legality does not exhaust morality. Moreover, there is no professional moralist, as there is a professional scientist. By and large, moral standards are formulated by various and sundry people. Clergymen, educators, statesmen, novelists, poets, newspaper editors, and columnists—all have a hand in the articulation of moral principles and rules. Very often such articulation comes as a result of spiritual or moral crises—wars, crimes, disasters, delinquency, sacrifices, heroic deeds, etc.

The most frequent problem confronting a moral agent or critic is whether a given rule ought to be so construed as to cover the action which he judges. Although a man is wholeheartedly committed to the rule of saving the life of another human being, he may have the problem of deciding the extent of danger or incapacity to help that justifies the suspension of the rule. Thus, in the process of reflection on the action, he reflects also cn the content and the scope of the rule. When there is a conflict between rules, he is to judge and to decide which rules are to be given greater weight in the context. This *active* relatedness of agents to moral rules is the most characteristic feature of moral life. In judging specific actions moral agents at the same time contribute to a further articulation of moral rules.

Where this is not the case, one may rightly suspect that there is something wrong. When the only thing that a moral agent needs to do in all his life is to see whether an action can be subsumed under what he considers a well-defined moral rule, he is either a saint or a blind conformist. Where moral rules are rigidly defined and precisely formulated, the reflection of moral agents will be confined to finding out what these rules are. The rules themselves will never be judged or reflected upon. But this is either a society of angels or a society whose moral fibre is atrophied. More likely it is the latter.

In a healthy community we can expect to encounter this active or creative relatedness of agents to moral standards. A scientist's problem is to introduce an intelligible order into phenomena which are *given* to him. But actions, into which a moral agent also seeks to introduce intelligent discrimination, are not given ; they are *to be done*. Consequently, moral goods of action cannot be secured and maintained without concern and commitment. Moral life needs conceptual frameworks in terms of which the moral worth of actions can be determined. As we cannot have a science of nature without a body of scientific theories which organize and explain concrete natural phenomena, so we cannot have morality without a body of rules and principles which articulate actual moral convictions. But this articulation is more than mere intellectual assent ; it also expresses active concern. Thus, moral concerns and commitments are the building blocks upon which any given moral structure must rest. This is also the reason why a universal moral code cannot be set up without an expression of a genuine concern for the moral goods which such a code would promote.

The Rice Institute

VI.—THE NATURE OF THE SENSE-DATUM THEORY

By E. M. Adams

THE kind of considerations offered in support and in refutation of the claim that there are sense-data reveals the sort of theory it is taken to be. The proponents of the theory have often argued as though it were an empirical matter. According to the sense-datum philosopher, Chisholm contends :

> The root of the problem lies in the fact that every perceptual experience contains *something* that can be made to vary independently of any of the physical things being perceived. . . . If you will look at the top of the coffee table, you will find in your experience something whose geometrical shape you can change merely by walking around the table. From here you experience something diamond-shaped ; if you were to stand directly above the table, you would experience something rectangular ; and, in moving from here to there, you could, if you chose, note the change occurring in the experienced shapes. . . . Similarly, merely by altering the conditions of observation in an appropriate fashion, you can vary experienced sizes, sounds, smells, and so on. In general, we may give the name ' sense-datum ' to anything in a perceptual experience which *could* thus be varied independently of the objects being perceived.[1]

If this is a correct view of the contention that there are sense-data, why all the fuss ? Why is it not a universally accepted truth, like, for example, " There is air " ? If one has not noticed them, he can be shown what is meant and that the claim is true. The critic does not deny that variations of the kind described occur. But he does object to the claim that there is *something*, a *thing*, in a perceptual experience that varies independently of the physical objects being perceived.

Professor W. Barnes, in an article entitled, " The Myth of Sense-Data ",[2] says that the sense-datum theory " makes the astonishing claim that we have all failed to notice a quite peculiar kind of entity, or at least have constantly made mistakes about its nature ". He gives three arguments to show that sense-data are " objectionable existents " and " wholly fictitious entities " :

> (1) Unlike physical objects they do not always obey the law of excluded middle. . . . So far as I can see, all so-called sensa . . . are indeterminate in this way. . . .

[1] Roderick M. Chisholm, " The Theory of Appearing " in Max Black, ed., *Philosophical Analysis* (Ithaca, New York : Cornell University Press, 1950), p. 103.

[2] *Arist. Soc. Proc.*, xlv (1944-45), 89 ff.

(2) It is a necessary consequence of the fact that a sense-datum *is* what it appears to be that there is no possibility of making further discoveries about its nature.

(3) Those who believe in sensa tell us so little about the laws of their existence that we are at liberty to make a variety of assumptions on quite fundamental points. For example, . . . If I blink my eyes while looking at a red patch are there two sensa separated in time, or is there only one interrupted in its career ? . . . There is nothing which enables us to consider one alternative nearer the truth than the other. . . . So we find them to have another peculiar property for existents ; they are not numberable.

So Barnes concludes : " Modes of appearance are clues to the nature of what exists, not existents."

Note that Barnes's criticism turns upon the point that the supposed sense-datum, the peculiarly varying factor or element in the perceptual situation, does not qualify for being counted a *thing*, an existent, but is rather a *mode* of perception. Thus the issue is conceptual rather than empirical. It concerns how we are to categorize the varying element. But he does not hesitate to conclude that there are no such *things* as sense-data, and that the theory is clearly false. The error charged is that of mistaken identity, a category mistake.

Other critics have sought to explain this category mistake, the taking of a mode of perception to be an object, a thing, an existent, by another category mistake. H. A. Prichard argues that it is based on the fallacy that perceiving is a kind of knowing. He contends :

if we go behind the mere verbal form of the statement : ' The colour which I see is a sense-datum ', we have to allow that it is only a misleading way of saying : ' My seeing the colour which I am seeing is a special kind of way of knowing it.'[1]

So, according to him, the validity of the sense-datum theory turns upon the question : " Is perceiving a kind of knowing ? " If it is, in every case of perceiving, by virtue of the nature of knowing, there must be *something* known that is distinct from and independent of the knowing. What is the *thing* known in perceiving, then ? Sense-data, according to the theory under examination. But Prichard maintains that seeing a colour, hearing a sound, and so forth, as modes of perceiving, are not cases of knowing. He gives two arguments to support this :

(1) What we perceive is always some secondary quality or qualities. Any secondary quality which we perceive depends on our perceiving it. Consequently to perceive something cannot be to know it, because if it were, the thing perceived would be independent of our perceiving it, and yet, being a secondary quality it is not (p. 209).

[1] *Knowledge and Perception* (Oxford: at the Clarendon Press, 1950), p. 211.

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Other critics have sought to explain this category mistake, the taking of a mode of perception to be an object, a thing, an existent, by another category mistake. H. A. Prichard argues that it is based on the fallacy that perceiving is a kind of knowing. He contends :

if we go behind the mere verbal form of the statement : ' The colour which I see is a sense-datum ', we have to allow that it is only a misleading way of saying : ' My seeing the colour which I am seeing is a special kind of way of knowing it.'[1]

So, according to him, the validity of the sense-datum theory turns upon the question : " Is perceiving a kind of knowing ? " If it is, in every case of perceiving, by virtue of the nature of knowing, there must be *something* known that is distinct from and independent of the knowing. What is the *thing* known in perceiving, then ? Sense-data, according to the theory under examination. But Prichard maintains that seeing a colour, hearing a sound, and so forth, as modes of perceiving, are not cases of knowing. He gives two arguments to support this :

(1) What we perceive is always some secondary quality or qualities. Any secondary quality which we perceive depends on our perceiving it. Consequently to perceive something cannot be to know it, because if it were, the thing perceived would be independent of our perceiving it, and yet, being a secondary quality it is not (p. 209).

[1] *Knowledge and Perception* (Oxford: at the Clarendon Press, 1950), p. 211.

(2) . . . if perceiving were a kind of knowing, mistakes about what we perceive would be impossible, and yet they are constantly being made, since at any rate in the cases of seeing and feeling or touching we are almost always in a state of thinking that what we are perceiving is various bodies, although we need only reflect to discover that in thinking this we are mistaken.

He concludes :

So far then from its being, as Professor Price asserts, certain that there are sense-data, it is certain that there are not (p. 213).

Professor Gilbert Ryle claims to refute the theory with a somewhat similar argument, but locates the category mistake differently :

I shall try to prove that this whole theory rests upon a logical howler, the howler, namely, of assimilating the concept of sensation to the concept of observation. . . .[1]

He argues :

Having at least one sensation is part of the force of 'perceiving', 'overhearing', 'savouring', and the rest. It follows that having a sensation cannot itself be a species of perceiving, finding or espying ; . . .

for if it were, we would be caught in an infinite regress.

If having a glimpse of a horse-race entails having at least one sensation, then having a glimpse of colour patches must again involve having at least one appropriate sensation, which in its turn must be analysed into the sensing of yet an earlier sensum, and so on for ever.

Also, he argues :

that there are several salient differences between the concepts of sensations and those of observations, scrutinising, detecting and the rest, which are revealed by the uninterchangeability of the epithets by which the different things are described. Thus we can speak of the motives from which a person listens to something, but not of the motives from which he has an auditory sensation ; he may show skill, patience and method in peering, but not in having visual sensations. Conversely tickles and tastes may be relatively acute, but his inspections and detections cannot be so described. It makes sense to speak of someone refraining from watching a race or of his suspending his observation of a reptile, but it makes no sense to speak of someone refraining from feeling a pain, or suspending the tingle in his nose.

Sensations then, Ryle concludes :

are not perceivings, observings or findings ; they are not detectings, scannings or inspectings ; they are not apprehendings, cognisings, intuitings or knowings. To have a sensation is not to be in a cognitive relation to a sensible object. There are no such objects. Nor is there any such relation. Not only is it false that sensations can be objects of observation ; it is also false that they are themselves observings of objects.

So these critics conclude that the sense-datum theory is grounded in a double category mistake and that with the exposing

[1] *The Concept of Mind* (New York : Barnes and Noble, Inc.), pp. 213-215.

of the fallacies involved the theory is duly disposed of. The arguments may be persuasive, but what of their validity ?

In the first place what do they prove ?   I think that they prove that sense data are not physical objects and that sensing, in the sense-datum theory sense, is not like perceiving or observing physical objects.   It is assumed that if an object or existent is not like a physical object, it is not an object or existent ; and that if sensing is not like perceiving or observing, it is not a form of knowing.   They have taken these assumptions from the framework of common-sense language in which to be an object or existent is to be physical and to observe is to observe physical objects or occurrences.   So the conclusions are not at all startling, and I suspect that they are unconvincing to any clear-headed sense-datum philosopher.   To conclude that there are no sense-data and that the theory is a myth grounded in a double fallacy is to misunderstand what the sense-datum philosopher is about.

The refutations given by Barnes, Prichard and Ryle are based upon the assumption that the sense-datum theory purports to be an empirical theory constructed within the common-sense conceptual framework.   Although many sense-datum philosophers may have talked this way, I do not think that the theory has ever been that.   A. J. Ayer, G. A. Paul, and H. H. Price (in some of his later writings) seem to have a more adequate view of what is involved.   They hold that the sense-datum theory is simply an alternative language to that of the language of ' it seems . . .', ' it appears . . .', ' it looks like . . .' and so forth, commonly called the language of appearing, which is an adjunct of the physical-object language.   The philosophical theories of perception, according to Ayer, are :

> not a number of alternative hypotheses concerning the nature of the empirical facts, but a number of alternative recommendations concerning the way in which we are to describe them.   And whether or not we are to accept any of these recommendations is a question that we have to decide on linguistic grounds.[1]

Paul speaks of the sense-datum language as an alternative way of describing the known facts.   He says :

> The word ' sense-datum ' as people have employed it does not fall into a fully-prepared scheme for its usage as a word for a physical object does, but its usage is not purely arbitrary . . . its use is connected with the use of certain words, which are in ordinary language, e.g. ' looks ', ' appears ', ' appearance ', and certain uses of ' this ', ' after-image ', ' image '. . . . If it is true that the surface which I see is round and brown, and looks to be elliptical and red, then according

[1] A. J. Ayer, *The Foundations of Empirical Knowledge* (New York : The Macmillan Company, 1940), p. 55.

> to our rule it is true to say that the corresponding sense-datum *is* elliptical and red.[1]

Price also, in at least one place, subscribes to the alternative language view.[2]

However, all three seem to think that it is *simply* a matter of alternative languages or notations. The one set of known facts or experiences may be described in the alternative ways. Ayer says :

> We find that every conceivable experience, in the field to which these theories refer, can equally well be subsumed under any of them. Each of them will cover any known fact (p. 53).

Again : " As languages, they afford us the means of describing what we already know . . ." (p. 55). Paul, in speaking of the fact that if a round and brown surface looks elliptical and red it is true, according to the rules of the sense-datum language, that the corresponding sense-datum is elliptical and red, says :

> Those who have in practice used the word ' sense-datum ' have not spoken as if what they were doing was introducing *merely* an alternative way of saying this same thing over again, but as if this new sentence which they substitute were in some way nearer to the facts. They have the idea that in some sense when a physical object looks red to someone then something really is red, *i.e.* that there really are in such cases two *objects*, one which looks red and one which *is* red, and that somehow the one which *is* red has generally been overlooked, and its existence now, for the first time been recognized (p. 107).

" I wish to deny ", Paul writes, " that there is any sense in which this terminology is any nearer to reality than any other which may be used to express the same facts " (pp. 108-109). Price's later view is similar. " I want to suggest ", he says, " that the appearing philosophy and the sensation philosophy (once each party has perfected its technique) are just two different ways of saying the same thing. . . . Either . . . will cover all the facts . . ." (pp. 227, 232).

These men do not mistake the sense-datum theory for a fallacy-bred empirical theory like the other critics. So they do not claim that the theory is either true or false, that there are or that there are not sense-data in any straightforward sense. However, Paul does seem to say that there really are no sense-data even though sense-datum sentences may be true. He says :

> I wish to deny that in order to give a complete and accurate account of any perceptual situation it is necessary to use a noun in the way

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[1] G. A. Paul, " Is there a problem about sense-data ? " in Anthony Flew, ed., *Logic and Language*, i, 106-107.

[2] " Seeming ", *Aristotelian Society*, Supplementary vol. xxvi (1952).

in which ' sense-datum ' is used, for this leads to the notion that there are entities of a curious sort over and above physical objects which can ' have ' sensible properties but cannot ' appear to have ' sensible properties which they have not got (p. 109).

Although the alternative language theory is a more adequate account of the nature of the sense-datum theory than the empirical view, it does not go far enough. I wish to suggest that the sense-datum theory is not simply an alternative notation to that of the language of appearing for describing a neutral set of facts that obtain and can be known independently of the language employed. It embodies an alternative ontology. It is an alternative categorial system which, if genuinely employed, provides the categories in terms of which the experience to be described is delineated. Kant maintained that the given of sensation is synthesized and ordered to form the world of experienced objects by the *a priori* forms of the mind. C. I. Lewis, in *Mind and the World-Order*, relativized the Kantian categories so that according to him alternative categorial systems are possible. But he seems to think that there is a common given element that constitutes what may be called ' experience$_1$ ' that is subject to being categorized in the alternative ways to yield a variety of world-orders or what may be called ' experience$_2$ '. If I understand him, experience$_1$ consists of sense-data. If this is so, sense-data transcend the relativity of categories. What I am suggesting is that this is not so. Categorization takes place at a more fundamental level. The so-called sense-data that are supposed to be the given are themselves the product of a categorization of a more primitive stuff of experience that is never presented uncategorized and may be subject to alternative categorization, for example, in terms of the categories of the language of appearing.

So sense-data, according to the view I am suggesting, are not *entities* that we discover and recognize in the manner in which we do a new kind of animal or rock ; or a new colour, or noise ; not even like the new kind of perceptions had by a person, born blind, who becomes able to see in adult life. Neither are they simply *things* in the role of being described by one language, which might equally well be described by another. The " discovery " of sense-data, and some have allegedly made such a discovery, is more like, and yet quite different from, the discovery of a new constellation in the sky (making a new grouping of what was there already), or that a sketch of a rabbit's head is suddenly perceived as a sketch of a duck's head. None of these illustrations is very good, but I cannot do better. What I am getting at is that experience, regardless of the level, comes delineated and ordered

according to our conceptual framework; vary the conceptual framework and experience is delineated and ordered differently.

I do not think that one needs to argue that we can delineate the stuff of experience, to some extent at least, in terms of the sense-datum category. I suspect that most of us who have seriously and sympathetically read the sense-datum philosophers have found ourselves doing it. And if we do so categorize the stuff of experience, there is no point in saying that there really are no such queer objects as sense-data. Those who do say it may mean that while operating within the framework of the physical-object language and its adjunct, the language of appearing, there are no sense-data and that this is the only framework possible. If so, I disagree with them only with regard to the latter. But I am not sure how I could show that they are wrong on this, otherwise than by pointing to alternative categorial systems, one of which is the sense-datum framework itself; and to suggest that they sympathetically adopt the theory of alternative frameworks to see if it does not give them important insights and clarification on a number of philosophical problems. If they should reject all the alternative frameworks I could point out, and either refuse to give the theory of alternative categorial systems a sympathetic trial run, or find no insights or clarification by doing so, we would be at an impasse. There is no " proof " that I could give that would convince them. However, I could attempt to show how the position I am suggesting might meet their arguments that there are no sense-data.

Within the sense-datum framework, the category of *object*, *thing*, or *existent* is reconstructed so that Barnes's objections to sense-data as objects or existents miss their mark, just as Spinoza reconstructed the category of substance so that the things ordinarily considered substances were not so classified by him. Actually not much reconstruction is needed. Barnes complains that sense-data are indeterminate and that the concept of *sense-datum* is so loose or vague that we cannot count sense-data. In our common-sense framework, the notion of physical object is so vague that we have to decide whether to count mirror images, shadows, rainbows and so forth as physical objects. So in a sense he might complain that we cannot count physical objects. Also some shadows, images, and so forth are just as indeterminate as sense-data. Another objection is that we cannot find out more about particular sense-data by investigating them further. There are many physical objects that are of such limited duration that we can have only one go at them. So it seems that the common category of object, thing or existent will almost do for the

sense-datum framework. But the sense-datum philosopher has no scruples against radically retailoring it to fit his needs if necessary.

Also the epistemological category of apprehending, cognizing or knowing is sufficiently amended in the sense-datum framework to embrace sensing within its range. Here again the ordinary concept of *cognizing* or being *aware of* does not need radical revision to do the job. Ryle, in charging that the sense-datum theory assimilates the concept of sensation to the concept of observation, assimilates the concept of what Prichard calls " perceiving secondary qualities " to the concept of sensations such as aches and pains, itches and tickles. What seems most appropriately called " sensations " are localized in the body. A piercing pain in a tooth, a cooling sensation, a burning sensation, a pricking or cutting sensation, a tingling or " pins and needles " sensation, an itching sensation, a falling or moving sensation— these are sensations proper. We *have* them. We are not tempted to think of them as apprehendings, cognizings, or knowings. We do not speak of their objects. They are all localized in the body or felt by the body as a whole as in the case of falling or moving. When one sees a colour or hears a sound, there is no sensation localized in the eye or in the ear. One takes note of something that seems to be out there some distance from him. Tasting and touching and the like do not seem to be quite as distinct from sensations as seeing and hearing, but even in regard to these there is phenomenologically an object aspect that is absent in the more typical sensations. So one wonders if to treat " seeing " colour patches and " hearing " sounds and, in general, to speak in sense-datum terminology, to treat sensing as a kind of knowing or cognizing somewhat like and yet quite different from observing chairs and tables and horse races, is any more an assimilation of different things than to treat sensing colours, sounds, and so forth as though they were just the same kind of things as pains, tickles, itches, tinglings, and the like. To assimilate " sensing " to either observation or sensation seems to be an error. There is a three-way distinction to be marked. Ordinary language has not been too tidy. The sense-datum philosopher is not cutting categories out of whole cloth or hopelessly confusing what is kept separate in ordinary language when he marks the distinction with the technical term ' sensing ' and thinks of it as a kind of knowing or cognizing.

But what does he do to the concept of knowing or cognizing ? Does he have to radically reconstruct it in order to get sensing into its fold ? We have already observed that in what the sense-datum philosopher calls ' sensing ' there is an object aspect. It

is the sensing *of* a colour, *of* a sound, *of* a shape, or *of* a cluster of
these even though there is no physical object, as in the case of
hallucinations or dreams. The sensing is simply an awareness of
the object. It is argued that sensing in this sense cannot be an
apprehending or cognizing since its object is not distinct from and
independent of the awareness. It is true that we cannot peer
at it from different perspectives or manipulate it in such a manner
as to determine its independence in the way in which we can a
physical object. But may not these features of cognizing a
physical object be attributable to the peculiarities of physical
objects and not to the nature of cognizing as such ? At least
according to the sense-datum philosopher, cognizing is simply
being aware of something. We are aware of different things in
different ways. One cannot be aware of a physical object in
the way in which one can a sense-datum. Some sense-datum
philosophers have concluded from this fact that one cannot be
aware of or cognize physical objects at all. He is just as much
and just as little justified in this claim as the critic who claims
that one cannot cognize a sense-datum because he cannot do it
in the manner in which he does a physical object.

The philosophical question, " Are there sense-data ? " is not
one that can be given a straightforward " yes " or " no " answer,
for it is not a straightforward question. It may be either what
Carnap calls an " internal " or an " external " question.[1] Em-
ploying a framework of thought that provides for talking about
entities of a given kind K, one can ask, " Are there any K's ? "
For example : " Are there tigers in Africa ? " " Are there chairs
in the room ? " " Are there numbers that are multiples of three
between nine and twenty-one ? " These are internal questions
and straightforward answers can be given. The same kind of
internal question can be asked in regard to our ontological
categories : " Are there physical objects ? " " Are there minds ? "
" Are there sense-data ? " Moore's famous hand-argument to
prove that there are physical objects is a straightforward answer
to the first question. While we cannot point out an X that is a
mind by physical gestures for visual perception, we can by other
means point out one that answers the internal question for the
mind category in essentially the same way. We might indicate
to a person who accepts the mind category an X that is a mind
by description : the mind that thought out (or thought up) the

theory of relativity ; the mind that devised the plan for Lincoln's assassination ; and so forth.   To the person who accepts the mind category such an argument is just as convincing as Moore's hand-argument is to those who accept the physical-object category.   Also the person who accepts the category of sense-data can answer our question, " Are there sense-data ? " in a similar manner.   Of course, we cannot point out a sense-datum in exactly the same way as we can a physical object, but to one who accepts the sense-datum category an X that is a sense-datum can be pointed out.   Moore attempted to point them out to his audiences.   On one occasion he did it this way : " I hold up this envelope, then : I look at it and I hope you will look at it. . . . What happened to each of us, when we saw that envelope ? . . . I saw certain sense-data : I saw a whitish patch of colour, of a particular size and shape . . . you also saw certain sense-data ; and I expect also that the sense-data which you saw were more or less similar to those which I saw." [1]   Chisholm (*op. cit.* p. 103) might attempt to point out sense-data as the elements in perception of a coffee table that can be made to vary by changing the conditions of observation.

This all looks very elementary and simple.   But why do intelligent people go on asking : " Are there physical objects ? Are there minds ?   Are there sense-data ? " and so forth ?   It must be that their questions are different or else they would settle for the obvious answers.   The existential question about a category, when asked as a philosophical question, is an *external* question.   It is not a matter of whether the specified class is devoid of members, but rather a question about whether to delineate the stuff of experience in terms of the category in question.   So, according to Carnap, they are not theoretical questions with answers that have truth-values.   Only internal questions are such.   But they are not meaningless or pseudo-problems.   He interprets them as practical questions asking about the advisability of adopting the framework.   This is a matter of the fruitfulness and efficiency of the framework for the purposes for which it was devised.   I presume that this means, do the internal questions formulatable and answerable in terms of the framework, more adequately describe and explain our experiences and guide our actions than those of its alternatives, in our attempts to come to grips with reality ?   This is obviously a very vague sort of test and the standards by which the outcome is judged often are not independent of what is being judged.

[1] *Some Main Problems of Philosophy* (New York : The Macmillan Company, 1953), pp. 30, 32.

15

The special business of philosophy, as I conceive it, is to study the categorial part of our common-sense and scientific conceptual frameworks. Analytic philosophy is concerned with clarification, explication, and internal criticism. Speculative philosophy, on the other hand, is concerned with external questions. It asks whether the categories of our system are adequate, or as adequate as some possible alternative. Speculative philosophers often propose new categories. As Whitehead said in *Process and Reality* (p. 4) : " Speculative philosophy is the endeavour to frame a coherent, logical, necessary system of general ideas in terms of which every element of our experience can be interpreted."

The sense-datum theory was not proposed because of the discovery of some new kind of entity in the ordinary sense. Neither was it offered as an elucidation of our common-sense ontological categories. The criticisms cited above miss their mark precisely because they are based upon the assumption that it was, or must be, one or the other of these two things. I suggest that the sense-datum theory is a *proposed* categorial system, a speculative philosophical theory, designed to render consistent what was believed to be an incompatibility in the basic epistemological and ontological categories of the common-sense framework, namely, an incompatibility between what it is to know and what the external world is so that knowledge of the external world becomes highly problematic if not impossible. Its merit is to be appraised and the external question, " Are there sense-data ? " answered by (1) a consideration of the genuineness of the problem of categorial inconsistency that it is supposed to solve ; (2) if the problem is genuine, whether it solves it better than any other theory ; and (3) whether it delineates the stuff of experience in such a way that we can come to grips with " reality " in a more satisfactory manner than we could with any other possible set of categories. I make no attempt in this paper to appraise it. My only concern has been to get at the kind of theory it is, and how it is to be judged.

University of North Carolina

VII.—PHILOSOPHY AND THE PRACTICE OF LAW

J. Z. KRASNOWIECKI

IT can safely be said that philosophy is not a popular subject among practising lawyers. Many lawyers feel that philosophy is a strictly intellectual exercise of no practical value to them. I do not think that this feeling is justified. I think that it exists largely because of the mistaken belief that philosophy deals only with fundamentals. And so, traditionally, the philosophical approach has been reserved almost exclusively for illuminating the so-called fundamental conceptions of the law. It is true that fundamental conceptions must be important, if they are fundamental. But this is little comfort to the lawyer who has to reduce them to concrete application in a climate which is not too friendly towards such exercise.

What I want to do here, is to stake out a narrower claim for philosophy in the law. I want to confine the enquiry to specific cases as they have come up in practice. My hope is that this approach will not only be more responsive to the practising lawyer's needs but also that it will arouse the philosopher's interest in the law.

Let us take the case of an accused who has pleaded not guilty to a charge of armed robbery, and, on the stand, has consistently denied that he had been present at the scene of the crime at the time of its alleged commission. Several witnesses (one of them not his friend or anyone related to him) testified, on his behalf, that he was in another place with them at the critical time. For the prosecution, several eye-witnesses to the crime testified that the accused is the man they saw committing the crime, but there was, in their demeanour, some slight suggestion of doubt. The only other evidence connecting the accused with the crime was that the getaway car was proved to be his. But the accused sought to explain this by producing evidence that in his community it is a common occurrence for one person to " borrow " another's car without permission, and he testified that he had discovered his car missing on the critical day but that it was returned the next day. Under the criminal procedure of the jurisdiction where this trial was held both defence and prosecution counsel are required to submit to the court a list of instructions which they propose should be given to the jury. The court then chooses the instructions that it will give.

Defence counsel submits the following instruction on the issue of accused's identity :

" A "   The court instructs the jury, as a matter of law, that before the accused may be found guilty of the crime charged, the evidence in this case must satisfy you beyond all reasonable doubt, that the accused was the person committing the crime charged, and if, after considering all the relevant evidence in this case, you should have any doubt remaining as to the accused's identity with the person committing the crime, the law requires that you should resolve this doubt in favour of the accused, and it is then your duty to find the accused not guilty. (The letter " A " has been inserted for easier reference in the text.)

Prosecution submits the following instruction on the same issue:

" B "   (a) The court instructs the jury, as a matter of law, that the identification of one stranger by another is generally a matter of opinion and belief. (b) If you find from the evidence in this case, considering the means of identification the circumstances in which the defendant was identified, the opportunity for identifying said defendant and the probabilities or improbabilities that it was the defendant, and after judging and weighing the testimony it does not satisfy you beyond all reasonable doubt that the defendant was correctly identified as the person who committed the offence charged in this indictment, it will be your duty to find the defendant not guilty. (The letters have been inserted for easier reference in the text.)

Suppose that the law is well settled, that an accused may not be found guilty unless his guilt is established " beyond all reasonable doubt " and that the cases indicate that instruction " A " is correct on the issue of identity but that there is no case which indicates whether instruction " B " is correct or not. The court accepts proposed instruction " B " and rejects " A ". The jury after being given instructions, including " B ", find the accused guilty.

On appeal, what arguments can defence counsel make to show that the giving of instruction " B " was prejudicial error ?   Let us suppose that there are available to him the results of a certain finite number of experiments with " typical " juries, which were designed to test the effect of these instructions. Let us further indulge in the fanciful supposition that the situation under which each of these experimental juries was tested was *identical* to that obtaining at the real trial, the only difference between these test cases and the real case being in the persons comprising the jury. Suppose then that in every case in which instruction " A " was

given, the verdict was not guilty, and in every case in which instruction " B " was given, the verdict was guilty. Since we know that instruction " A " is correct under existing law it would follow that instruction " B " is defective. It would follow because we would have enough evidence to say that had instruction " A " been given to those experimental juries to whom instruction " B " was given, they would have returned the verdict not guilty, in the same way as did the juries to whom instruction " A " was given. We could then carry this hypothesis over to the real case. It is important to note that this hypothesis, the state of inquiry into human motivations being what it is today, would not be as strong as would be a hypothesis, based on roughly equivalent evidence, with respect to the behaviour of things. However, without assessing the strength of our hypothesis, we would be justified in rejecting instruction " B " on the grounds that the law is designed to cover all cases. But, then, the question arises, how many instances of disparity in result as between instruction " A " and instruction " B " do we need before the idea which is expressed in the sentence " The law is designed to cover all cases " begins to take effect ? That is, suppose that the result of the experiments was that in all cases but one, whichever instruction was given, the verdict was not guilty. But in that one case, a case where instruction " B " was given, the verdict was guilty. Could we legitimately put forward the hypothesis that if, in that one case, instruction " A " had been given, the verdict would have been otherwise ? Surely not. Suppose two such cases of guilty under instruction " B " in a run of not guilty findings under both instructions. Would two be sufficient to persuade us ? And if not, where would we draw the line ? Suppose that the result of the experiments was that there were roughly the same amount of convictions and acquittals under instruction " A " as there were under instruction " B ", but that under instruction " B " there were one or two extra findings of guilty. The same question would arise, where should we draw the line ? I do not see how the answer to this question can ever be shown to be correct.

This may not be fatal to the contention that such a method of determining the correctness of instruction " B " is ideally the best that is available to us. It might be that we should find a certain uniformity of opinion amongst us as to where the line should be drawn. But even if this line can be drawn with a sufficient measure of agreement it will be a very odd kind of line, it will not be a dividing line at all. My grounds for saying this are as follows : Suppose the result of the jury tests has been that

in every case, regardless of whether instruction " A " or instruc-
tion " B " was given, the verdict was not guilty.   Would this
result have any relevance to the question whether instruction
" B " is good or bad ?   I think the answer is, none.   To test this
we must ask the further question : Is the doubt that the result of
any finite number of experiments with "typical" juries holds good
with the nth jury, of such a strength that were a doubt of the
same strength to be entertained with respect to the existence of
any of the elements of the accused's guilt, he ought to be ac-
quitted ?   I do not think that I can get any closer to the point .
that I am trying to make, than to state my belief that most
people would say " yes ".   And if this is so, a way is open to the
acceptance of my conclusion that jury experiments having that
result, that is one where, whichever instruction is given, the verdict
is not guilty, would have no relevance to the question whether
instruction " B " is good or bad.   It would have no relevance
because while the doubt that the experience in a certain number of
cases will repeat itself in the nth case, our case, may not be strong
enough to persuade us that instruction " B " is bad, it is, if the
answer to my last question is admitted, strong enough to prevent
us from holding it good.   The same obtains for the result where
there are roughly the same amount of convictions and acquittals
under instruction " A " as there are under intruction " B ".
But these are the extreme cases " this side " of the line, about
which we have spoken.   What holds good for the extreme case
must hold good for all other cases " this side " of the line.   It is
true that results falling the " other side " of the line would be
relevant to the question whether instruction " B " is bad.   There
could, however, be no result that would show the instruction to
be good.   Thus the usefulness of such experiments depends upon
the fortuitous circumstance that a certain result is obtained.

A more basic objection to the empirical method of determining
the difference between instruction " A " and instruction " B " is
that there is a simpler and more appropriate way of determining
it.   For that difference, if there is one, lies in the *meaning* of each
instruction.   And on the question of meaning, it is not only
troublesome and costly to go out and watch how a finite amount
of individuals react to a given set of words, but it is more ineffi-
cient and more dangerous than just sitting and thinking.   It is
inefficient because it is unreasonable to believe that the conditions
of one individual's reaction can be exactly repeated with another,
and it is dangerous because there is a strong temptation, should
a set of irrelevant experiences be obtained, to force them to yield
a conclusion.

I have said that just sitting and thinking is a more appropriate way for determining the difference between the two instructions. My meaning here can be illustrated by considering a recent case in the Court of Appeal, *Hornal* v. *Neuberger Products Ltd.* (1956) 3 W.L.R. 1034. Plaintiff, Jimmie Hornal, a precision engineer, wanted a Herbert No. 4 capstan lathe to carry out certain contracts for work. He heard that the defendant, Neuberger Products Ltd., had such a lathe for sale. He went to inspect it at their premises, where he saw it in operation, talked to the director Mr. Neuberger, and agreed to buy it. After taking delivery of the machine, he discovered that a part of it—the turret—was defective, and he had to remedy this defect at a cost of £50 14s. 3d. He thereupon brought this action against the company, claiming damages for breach of warranty, and in the alternative, damages for fraud. As his basis for this action he alleged that during his conversation with Mr. Neuberger, the latter had said to him, " The machine has been Soag reconditioned ", a statement, which if made, was not true. Now in order to succeed on the ground of breach of warranty it is necessary to show (1) that the statement was made, (2) that it was meant to be and was part of the contract for sale, and (3) that damage resulted from the fact that it was false. But in order to succeed on the ground of fraud it is necessary to show (1) and (3) and instead of (2) it must be shown that the statement was known to be false by its maker.

The trial judge first considered the ground of breach of warranty, and he found that the evidence established (1) and (3) but did not establish (2) and he found against the plaintiff on this ground.

Traditionally, at common law, there are two standards for determining the sufficiency of evidence on any question of fact :

The civil standard is : Am I satisfied *on the balance of probabilities* that the fact sought to be proved has been proved ?

The criminal standard is : Am I satisfied *beyond all reasonable doubt* that the fact sought to be proved has been proved ?

Now, on the issue of breach of warranty, the trial judge was dealing with an issue as to which it is established that the civil standard is appropriate, and the judge said as much.

But when he came to the issue of fraud, he was dealing with an issue as to which the criminal standard of proof prevails. So that when the judge came to (1), the question whether Mr. Neuberger had said the " machine has been Soag reconditioned ", he said : " If I have to be satisfied beyond all reasonable doubt I say at once that I should not be so satisfied in regard to the statement that it was Soag reconditioned " (1956, 3 W.L.R. 1034 at p. 1037), and he held against the plaintiff on the issue of fraud.

Now we have the questions which confronted the Lord Justices
on appeal. Was there anything wrong with the way the judge
stated those problems to himself, *i.e.* instructed himself, so that he
found, on the issue of warranty that the statement was made, and
on the issue of fraud that it had not been made ? Plainly there
was not, because the law is well settled on the way in which the
standard of proof ought to be stated on each issue and the judge
made no mistake there.

But the next question which the Lord Justices had to answer
was, was there anything wrong with the way the judge interpreted
the *meaning* of those two standards of proof ? I will show in a
moment that this is the question which they asked. But let me
pause here to point out that this question is not one that can be
answered on the experience of how this judge interpreted the
meaning of those two standards, nor is it one that can be
answered on the experience of how a finite number of judges inter-
pret it, it is a question which calls for a logical analysis of their
meaning, not for experimental analysis at all. I do not mean to
suggest that there is a gulf between experience and logic such that
one has no relevance to the other but the advantage of approach-
ing the question of meaning by asking first what meaning can
logically be assigned to certain words, is that the inquiry does not
straightaway run foul of a range of emotive reactions that any
particular word might evoke. The importance of this observa-
tion, here, is that the question which the Lord Justices had to
determine was precisely the question whether the trial judge's
reaction to the two standards of proof was one which was con-
sistent with what meaning could logically be assigned to them.
The proof that this is the question which they had to determine
lies in the answer which they gave. If I may be allowed to
restate that answer, it is that these different standards are meant
to reflect the tendency among us to vary our certainty in the
truth of any proposition in accordance with the type of practical
result which its truth might have. Consider the case of a man
walking on the edge of the flat roof of a 20 foot building and
compare it with the case of the same man walking on the edge of
the flat roof of a 200 foot building. Let us exclude all the other
variables of these two situations, such as the difference in wind
velocity, the effect of height upon this man, etc. By excluding
all the variables except the height, we have made the probability
of fall identical for both heights, and if not, let us agree that we
have. We can say without experiment, that this man would
take more care at the 200 foot level than he would at the 20 foot
level. Now, of course, he would thus be decreasing the probability

of fall, but what persuades him to do this is not the greater
probability of fall but the probability of a greater fall.[1]   And this
is what the Lord Justices perceived when they addressed them-
selves to the meaning of the two standards of proof.   Denning,
L. J., said : " The more serious the allegations the higher the
degree of probability that is required " (1956, 3 W.L.R. 1034 at
p. 1041) and both Hodson, L. J., and Morris, L.J., quoted Denning,
L.J.'s statement in *Bater* v. *Bater* (1951) P. 35, 37, with approval,
that what is back of these standards is the " degree of probability
which is commensurate with the occasion ".   This view of the
process of how we become convinced of the existence of some fact
led the judges naturally to the conclusion that the two standards
draw their meaning from this process.   The question which they
did not answer, but which is answerable, is whether some further
meaning could not logically be assigned to them.   The answer is
that none is.   When I say that none is, I do not mean that a more
careful analysis of the process by means of which we become con-
vinced of the truth of any proposition could not be made ; what
I mean is that the two standards cannot be allowed any meaning
other than in terms of that process.   In particular, they cannot
be allowed any meaning which would permit them to enter into
this process of convincing, because that meaning could only be
emotive.   Again I do not mean that they do not in fact have
emotive meaning, but they cannot be allowed it, at any rate not
in the context in which it is desired to test the validity of any
particular reaction to them, as it was in *Hornal* v. *Neuberger*.
This being the way in which the judges in that case analysed the
meaning of these two standards, they came to the right conclusion
that, in a case where the possible results to the defendant are
roughly of equivalent gravity, the application of these two
standards each to the same question in the context of two different
issues, could not legitimately lead to a different finding of fact
according to which issue was being considered.

What is the importance of all this in our discussion of the two
jury instructions " A " and " B " ?   I think it is this : in the part
marked (*a*), instruction " B " tells the jury that " the identifica-
tion of one stranger by another is generally a matter of opinion
and belief ", and in the part marked (*b*), it tells the jury that the
accused's identity with the man committing the crime must be
proved beyond all reasonable doubt.   If we allow no emotive
meaning to part (*b*) it merely restates the process of convincing.
If we allow no emotive meaning to part (*a*) it is meaningless or

[1] For an interesting recognition of this, in a decision of the House of
Lords, see *Paris* v. *Stepney Borough Council* [1951], A.C. 367.

nearly so.　At best it conveys the insignificant information that no one can be as certain of recognizing anything as he can be of recognizing analytic truths.[1]　At worst it carries the emotive meaning that the witnesses need not really have been so very certain of recognizing the accused, and that the jury can be satisfied with something less than a display of knowledge on their part.　It can be stated with some degree of confidence that the jury would not search out the only and insignificant descriptive meaning that this sentence might have.　They would naturally accept it in the emotive sense.　The danger of this is sufficient reason that instruction " B " should be rejected.　It might be objected that if part (a) of that instruction is liable to be given emotive meaning, part (b) is too, and since that meaning in part (b) would have the tendency to tug the opposite way, the vice of part (a) would be cancelled out.

That may be allowed and yet it is clear that instruction " B " is defective.　We have started this discussion with the observation that instruction " A " is correct under existing law.　And even if there is no authority directly approving an identical instruction, its correctness, or the correctness of any similar instruction, would seem to follow from the general rule that every element of an accused's guilt must be proved beyond all reasonable doubt.　If part (b) of instruction " B " carries an emotive meaning then the equivalent passage in instruction " A " does too.　But in instruction " A ", or any other similar instruction, there is nothing equivalent to part (a) of instruction " B ", and thus nothing to do the cancelling which, it is supposed, takes place under instruction " B ".

This being the analysis, it might be asked what use could be made of it in court ?　I think one could say something like this :

" If it be allowed that my proposed instruction ' A ' is correct under the existing law, or even if it be allowed simply that the law requires that every element of the accused's guilt be shown to exist ' beyond all reasonable doubt ', then I respectfully submit that I can show that instruction ' B ' is incorrect and that a substantial miscarriage of justice occurred when this instruction was given to the jury.

" I would like to draw the Court's attention to the main difference between instruction ' A ' and instruction ' B '. Instruction ' B ' contains, at the outset, the words : ' The court instructs the jury as a matter of law that the identification of one stranger by another is generally a matter of opinion and

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[1] Cf. A. J. Ayer, The Problem of Knowledge (1956), esp. chap. 2 ;　Max Black, ' Justification of Induction ', Problems of Analysis (1954), pp. 157 ff.

belief.' These words do not appear in instruction ' A ', and they constitute what, I submit, is fatal to the validity of instruction ' B '.

" There may be a sense in which the recognition of any object or person is a matter of opinion or belief, but it is the sense in which anyone might say that what he has seen may not have really been there, or the sense in which I might now say that I only believe that I cannot walk through this court-room wall. Plainly it is not the sense in which the ordinary man uses the words opinion or belief. The ordinary man usually says that he knows that what he sees is there and that he knows that he cannot walk through a wall except by way of a door. And where memory is involved, as where he is asked to recognize something or somebody, the ordinary man says that he knows that he has recognized them, when he has done so. The ordinary man reserves the words opinion or belief for things that he is not so certain about. To an ordinary man believing that he has recognized somebody is different from knowing that he has done so. This shows, I submit, that the quoted words of instruction ' B ' would convey to the jury the impression that they are given a licence to be satisfied with something less than a display of knowledge, in the ordinary man's sense of that word, on the part of the witnesses who identified the accused in this case. It is true that instruction ' B ' goes on to caution the jury in terms of proof beyond all reasonable doubt. How can one tell whether this caution restores, to the degree of proof necessary in this case, what has been taken away in the earlier part of this instruction ? It is respectfully submitted that this Court ought not to speculate on a matter of such vital importance to the accused."

I do not insist that this analysis is complete, or that there are no " kinks " in it, but what I am concerned with here is to arouse the philosopher's interest in the law and the lawyer's interest in philosophy.

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VIII.—DISCUSSIONS

SOME CONSIDERATIONS ON MORRIS LAZEROWITZ'S "THE STRUCTURE OF METAPHYSICS"

PROFESSOR MORRIS LAZEROWITZ has recently brought together a number of his published and unpublished papers under the title *The Structure of Metaphysics*. The unpublished papers include the one entitled *The Nature of Metaphysics* which explicitly formulates the approach and the view that has been implicitly used in all the other papers. It will be our purpose in this paper to show that his view suffers from the same defect which he has tried to show in others and that his practice hardly accords with his theoretic convictions.

Lazerowitz's contention has both a negative and a positive aspect. Negatively, he has tried to show that Metaphysical theories are neither empirical nor *a priori* nor verbal nor just nonsense as supposed by many thinkers. Positively, he has argued that they are linguistic innovations, whose primary purpose is the disguised gratification of unconscious needs. Lazerowitz seems to think that his positive and negative proofs are intimately related to each other and does not consider the possibility of any other alternative being left in the field.

Lazerowitz's view of metaphysics is profoundly influenced by ' the chronic condition of endless and unresolved debates ' in the field and the fact that there seems no way of resolving these disputes to the satisfaction of any of the persons concerned. The paradoxical situation that philosophers sincerely hold positions which they *know* to be false (for example, that we cannot know other minds) and that they continue to hold such positions even when they are shown the contradiction involved in such a situation, has suggested to him the parallel with the behaviour of a neurotic who continues to believe and behave in the same manner even when all the objective evidence should have led him to leave and alter them. With full consciousness, therefore, he has taken the help of psychoanalysis in understanding the matter and has argued that in metaphysics " our intelligence has discovered an ingenious way of putting language into the service of emotional needs, and at the same time keeps its discovery a secret from itself " (p. 79). He is aware that the statements of any person may have private meanings for his unconscious, but the difference with the metaphysician's statements lies in the fact that it is their sole function. He writes : " Unlike the physicist who reports and explains physical phenomena, but whose statements may also have private meanings for his unconscious, the metaphysician can with hardly any exaggeration be said to use his statement solely to express unconscious material. It can almost without exaggeration be said that *his statement* expresses nothing

over and above, nothing in addition to, the unconscious material it
denotes " (p. 78, italics author's). The slightly hesitant phraseology
is only, so to say, a manner of speaking. The author's italics clearly
reveal that he has no doubt about the matter. In fact, metaphysics
is nothing but a dream with language and the metaphysician nothing
but " a verbal magician who is taken in by his own tricks " (p. 79).
He writes : " Like a dream, a metaphysical theory is a production
of the unconscious." "A metaphysical theory . . . is a verbal
dream, the linguistic substructure of which has to be uncovered
before we can see what it comes to and how it produces its effect "
(p. 26). " Indeed, . . . the metaphysician may be said to dream
with words " (p. 149).

Now it would be interesting to ask if Lazerowitz's theory about
metaphysical statements is empirical, a priori or verbal in nature.
Prima facie, it seems to belong to the first group. At many places,
he has explicitly stated that he is proposing a hypothesis and that
" it is certainly permissible to try out still another hypothesis "
(p. 149). He says that " it is now possible to establish them or
disestablish them for there does exist a science of the unconscious "
(p. 69).

His theory, then, is a hypothesis about the unconscious deter-
mination of the linguistic behaviour of certain persons known as
philosophers. Such a hypothesis can be submitted to the usual
verificational tests and be made more or less probable if no alterna-
tive hypothesis or hypotheses are as or more adequate to account
for the known facts. Lazerowitz must be aware that even in the
field of neuroses, the psychoanalytic hypotheses are not the only
ones that hold the field.

But if one accepts this view about the nature of the theory that
Lazerowitz has formulated and turns to his book, one is amazed to
find that there is hardly any evidence of empirical methodology
being used throughout the whole book. There are no data, either
statistical or clinical, on which the theory is supposed to be based.
In the whole book, there are only about eight pages (69-78) where
the author has tried to analyse the unconscious material behind
the metaphysical statement " Change is unreal ". This analysis,
however, is not based on any case-history material that he gathered
in his psychoanalytic investigations, but only on the linguistic
statement that he has met with in the writings of some persons.
He seems to believe that the statement is a sufficient warrant for the
analysis and that the correctness of the analysis is not to be tested by
any clinical criteria, as in the case of psychoanalysis, but is inherent
in itself and can be intuited by anybody for whom it ' clicks ' as it
has done for the author. He writes : " The hypothesis I am going
to formulate . . . , to put the matter subjectively, has ' clicked '
intellectually for me " (p. 58).

Now, it will be admitted, we hope, by Mr. Lazerowitz that
" clicking " is not a sufficient condition for the establishment of any

empirical hypothesis, and he would be a strange scientist indeed
who is quite satisfied with a hypothesis just because it has " clicked "
for him. However intuitively certain the " clicking " may be for
the investigator, he has to devise ways and means to test it further.
It is also not the case that he has propounded a hypothesis which,
though theoretically verifiable, is impossible of being tested in the
present condition of our knowledge. The hypothesis belongs to the
well-known field of psychoanalytic theories and, whatever may be
the limitations of the verificational methodology in that field, there
certainly is a methodology to test the various hypotheses put forward
in that field. It is inconceivable that Lazerowitz does not know
this fact, yet it is equally strange that a serious thinker should
formulate an empirical hypothesis and not try to test it.

Perhaps Lazerowitz is too lazy to test his hypotheses. He is the
theoretician who suggests hypotheses which seem probable to him
and which are to be tested by those who work in the applied field.
If this is true, then his behaviour is puzzling in the extreme. A
theoretical hypothesis about a particular type of phenomena should
be communicated to those who are working in the field. But Lazero-
witz has not communicated his hypothesis to any psychoanalytical
journal where it might draw the attention of some practising psycho-
analysts who may test it in their clinics. In fact, he seems to
be suffering from what he himself has characterised as Moore's
paradox. From 1939 to 1952, he has been contributing a number of
articles to various philosophical journals including MIND, *Analysis*
and *The Philosophical Review* in the hope of convincing philosophers
of their mistakes. Even in the new volume, he has included two
new papers on " Appearance and Reality " and " Logical Neces-
sity ", hoping to convince the philosophers of the disguised wish-
fulfilment nature of their theories. But it is incredible that he
should not know that if his hypothesis about metaphysical state-
ments is correct, the philosophers could never be convinced by the
type of arguments he has been giving to convert them to his position.
And if a philosopher does really get converted after reading any of
Lazerowitz's papers, he obviously could not have been holding his
position due *solely* to unconscious reasons. Rather, it must have
been the confusions in his thinking which had led him to that position
and now that those confusions had been clarified by Lazerowitz,
there seemed no reason to hold it.

Lazerowitz has continuously used behaviour as a test of, or rather
as identical with, knowing in the course of his arguments. If we
apply this criterion to his behaviour, he should be taken to *know*
that his theory is *false* and yet to persist in holding that it is *true*. For
otherwise, he would have left philosophy long back and started as a
practising psychoanalyst to cure the philosophers of their meta-
physical wish-fulfilments. The only trouble would have been that
he would have hardly received any patient, for no philosopher is yet
known to have gone to a psychoanalyst for his metaphysical troubles.

The difficulty could perhaps have been avoided, particularly in these days of socialism, by asking the state to legislate for the avoidance of the huge national waste involved in so much of national intellectual capital being invested in unfruitful channels. An agitation for a compulsory psychoanalysis of the philosophers should have been launched long ago so that they could be freed from their obsessive verbal dreams and helped towards a better use of their intellectual abilities. Simultaneously with this there should have been a demand for the abolition of the teaching of philosophy in the universities. But, as far as we know, Lazerowitz has done nothing of the sort. Nor does it seem that he intends to do anything of the kind.

It may seem scandalous to urge such considerations against a theoretical position so subtly argued by a philosopher. But we wish to urge with the utmost seriousness that such behavioural consequences are implicit in the view of metaphysics held by Lazerowitz and certain other thinkers. If they feel uncomfortable about the consequences, they should reconsider the view that they have come to hold because of certain reasons.

But perhaps we have been mistaken about the empirical character of Lazerowitz's view about the nature of metaphysics. Perhaps, his theory has an *a priori* character and, therefore, stands in no need of verification through experience. He writes, for example, about " clicking " and thinks that nothing further can be done about it. Either it " clicks " for a person or does not " click " and " the fact that others do not see shows no more than that they do not see " (p. 58). This " seeing ", however, cannot be any physical " seeing " even though the example he gives is of that nature. He writes : " I mean something like this ; you look at a drawing in which there are hidden animals sketched into trees, bushes, the ripples in a brook, the contours of a hill, and so on, and suddenly you *see* them. That is all there is to it " (p. 58, italics author's). But obviously he could not mean that there is an analogous physical seeing of geometrically patterned physical objects in the case of his theory about metaphysics. The " seeing " could only mean metaphorically the type of ' seeing ' by which we see the truth of a statement such as " $2 + 3 = 5$ ". But such a truth is of an *a priori* kind and, thus, there may be some justification for the possibility that his theory about the nature of metaphysics is *a priori* in character.

But, according to Lazerowitz, " If the theories are about phenomena they cannot be *a priori*, nor have *a priori* demonstrations " (p. 44), and yet his theory is about the nature of metaphysical statements and the behaviour of philosophers with respect to them, the latter of which is certainly a phenomenon in his sense of the word. According to him, the logically necessary is that the opposite of which is inconceivable, but surely we can imagine what it would be for his theory to be false. It is not the case that the statement " Metaphysical statements do not *solely* express unconscious material " does not, or rather cannot, have a descriptive use.

According to his own views of the nature of *a priori* statements, therefore, his theory about the nature of metaphysics cannot be *a priori* in character, even though the method of his argument and the insistence on " clicking " may lead us to think otherwise.

If Lazerowitz's theory about the nature of metaphysics is neither empirical nor *a priori* in nature, does it then "in a concealed form, make factual claims with regard to established linguistic usage " ? Both the term " metaphysics " and the term " dream " have a fairly determinate usage in the English language, but, as far as I am aware, the term "metaphysical dream " has only a metaphorical but no literal sense. It will be a strange " dream " indeed which occurred only to a few persons and to them only when they were awake and in full control of their reasoning powers and lasted during the whole of their lives and involved a continuous debate with other persons through which it became modified. We have certainly heard such statements as " Our life is nothing but a long dream out of which we awake when we die ", but it was a poet's use and we were not expected to take it literally. Lazerowitz, however, expects us to take him literally, or does he not ?

Perhaps he is merely giving us an analogy. He only wants to draw our attention to certain similarities between a dream and a metaphysical theory. Both are products of the unconscious and their function is the same in both the cases, *viz.* disguised wish-fulfilment. He writes, for example, that the philosophical statement " is, so to speak, an exotic plant and requires a *special atmosphere* in which to live and flourish. In this respect it is like a dream, which requires *special* conditions, *e.g.* a temporarily weakened sense of reality, in order to play its role effectively in the mind of the dreamer. The great importance of the utterance to the philosopher makes it safe to compare it with a dream in still another respect : like a dream it is fundamentally a wish-fulfilment which can make its appearance against resistances only by presenting itself in a disguised form " (p. 227, italics ours).

It would be interesting to ask Lazerowitz as to the " special atmosphere " and the " special conditions " under which alone a philosophical statement can be made. Is a philosopher suffering from " temporarily weakened sense of reality " when he writes or thinks or engages in a serious discussion with other thinkers on the subject ? Of course, his mind, for the moment, is concerned only with his subject and not with the hundred and one other problems that exist in the world. But that is the situation of every scientist, artist or man of action when engaged on his particular problem. Does he then talk irrelevantly about his subject or does his mind wander aimlessly without coming to the point ? Or, does he lose his capacity to disengage himself from his subject and respond relevantly to other problems when they press for his attention ?

It will be difficult to answer these questions in the affirmative. Lazerowitz himself is aware that the philosopher behaves quite

normally in the ordinary day-to-day behaviour of life. Not merely
this, he would perhaps concede that a philosopher may think rele-
vantly in the various physical, social and mathematical sciences also.
But, does he then think irrelevantly only when he is concerned with
his own subject matter ? This is not very likely, and there are
always others in the field to point out if ever there is such a case.
The " atmosphere " in which a philosopher works is in no way
different from that required by any other theoretical thinker.
Obviously, the house must not be on fire and he must not be suffering
from excruciating pangs of hunger or pain, but these and " con-
ditions " like these are required for any activity that transcends the
biological level. The philosophical activity does not presuppose
any lessening of vital energy or the cessation of higher brain functions
which are generally supposed to be the necessary conditions for
any weakening of one's sense of reality.

It is impossible to think that Lazerowitz does not know of these
things. Ordinarily, he would, we hope, never say of a man who was
seriously arguing with him that he was " dreaming " or suffering
from " a weakened sense of reality " or from " neurosis ". But
when he writes a whole book seriously maintaining that it is so, we
can only suspect that he is using the words in a different sense.
That a philosopher's chief method of proving his proposition consists
in " disguised linguistic innovation ", is the contention of Morris
Lazerowitz in his book. Whether true or not of other theories, it
seems a peculiarly apt description of his own practice. The use of
the terms " dream ", " weakened sense of reality " and " neurosis "
are not meant to be descriptive at all, though the author continues
to use them as if they were so. The startling effect of his book,
however, relies on this very deception and both the author and the
reader are mostly unaware as to how it is being produced. But, as
he himself has written : " One cannot refute, nor for that matter
establish by proof, a language innovation. One can only like it or
dislike it. It can attract or repel, become popular, lose its popularity,
be forgotten, be revived, be forgotten again and revived again.
But it cannot be refuted or established, be *proved* false or *proved*
true " (p. 64, italics author's).

Though incapable of being proved true or false, the linguistic
innovation is supposed to satisfy deep emotional needs and it is
because of this reason that Lazerowitz contends ; " At a deeper,
psychological level our emotional needs blind us to what is being
done with language " (p. 66). It should be important, therefore,
to inquire about the " emotional needs " that lie unconscious behind
the linguistic innovations of Lazerowitz.   As he himself has written :
" What he (*i.e.* the philosopher) is actually doing is introducing a
linguistic innovation the contemplation of which gives him and
others pleasure. We can be sure that he gratifies himself uncon-
sciously with his sentence, that it works in behalf of unconscious
needs " (pp. 67-68).

16

The "unconscious emotional needs" behind Lazerowitz's linguistic innovations are not difficult to find. He wants to be in the line of Copernicus, Darwin and Freud, each of whom is supposed to have given an increasing blow to man's self-love and self-pride. He writes : " In a famous passage Freud describes three great ' outrages upon its naive self-love ' which cultured mankind has had to endure in the last few hundred years. And if the present hypothesis is correct in its general outline, a special group of intellectuals must endure still a further blow to their ego : it exposes these intellectuals, who have prided themselves on being impersonal seekers after truth, as the dupes of games they unconsciously play with language" (p. 226).

One can almost feel Lazerowitz relishing the blow that he has so deftly given against the " impersonal seekers after truth ". The gods have fallen from the pedestal and one can revel in the destruction all round. One can, if one so likes, "discover" other motivations and needs behind this which has so obviously been admitted by the author himself. The game is easy and can be played against anybody if one so desires. But has Lazerowitz ever stopped to ask himself if there is any such thing as " impersonal seeking after truth " ? If there is no such thing, his charge against the philosopher is superfluous. If, on the other hand, he admits that there is such a thing, he should have given the relevant criteria and shown how the philosopher fails to fulfil them. The " impersonality " is a characteristic of the " seeking " and not of the particular subject-matter with which the " seeking " is concerned with. Lazerowitz will have to tell us in what way the " seeking " of the philosopher is so different that it cannot, even possibly, be impersonal.

Lazerowitz's theory about the nature of metaphysics has, thus, a three-layer structure. At the conscious level of our minds, it causes " the erroneous idea that it states the existence or non-existence of a phenomenon or that the phenomenon has or lacks a certain property ". This erroneous idea " is itself a pre-conscious re-edition of a familiar term and expresses one or more unconsciously held beliefs, the purpose of which is to satisfy a repressed longing or to ward off a repressed fear " (p. 67).

I do not know if Lazerowitz will welcome this analysis of his theory about the nature of metaphysics. In a sense, he should—for there is no better proof of the validity of a theory than that it applies to itself. His is a philosophical theory about the nature of metaphysics and, thus, it should itself be of the nature which it alleges all philosophical theories to have [1] I suspect, however, that he would not

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[1] Lazerowitz has not drawn any relevant distinctions between " metaphysics " and " philosophy ". In fact, his theory about the nature of metaphysics is a theory about all philosophical statements and has, in this paper, been treated as such. Any attempt at a distinction will have to find cognitive meaning for a proposition which is neither empirical nor *a priori* nor verbal in character. He is precluded by his analysis from entertaining any such notion as this.

welcome the analysis that I have tried to give in these pages for the simple reason that, if correct, it will deprive his theory of any truth function. He is almost certain to feel the utter irrelevance of our analysis to his theory and be irritated at our crude mimicry of his subtle and acute argument. Such a feeling will, I hope, be all to the good since it might make him understand others' identical feeling with respect to his work. It should be noted that the theory of types can be of no help in the matter, as we are not alleging that the theory does not apply to itself, but only that it does.

If, however, Lazerowitz reflects ever so slightly on his dissatisfaction, he would discover the reasons for others' dissatisfaction with him. He has tried to convince us by certain *arguments* and instead of showing what is wrong with the arguments, we have started analysing their *nature* and the *psychological motivations* lying behind them. This may be interesting, but it is bound to seem irrelevant to the author of the argument. I should confess that I myself am not highly impressed by such sort of refutations, but if I have taken the trouble to play the game with Lazerowitz's work, it is only that he may come to share my dissatisfaction with the type of argument he so often employs against other thinkers' work.

K. DAYA

THERE is an argument (was it Smart who started this ?) that goes like this : Time cannot really flow or pass, for if it did it would make sense to ask at what rate it flowed or passed, and this would require a super-time for it to flow or pass in. I want to argue here that this argument, though it was worth trying once, is really either pedantic or perverse—pedantic if it is meant to show only that all talk of time's flow or passage is figurative, which everyone knows ; perverse if it denies the truth which this figure is plainly intended to convey, which everyone also knows.

A year ago Johnny celebrated his eleventh birthday, now he is celebrating his twelfth, and in a year's time, if he's spared, he will celebrate his thirteenth. The date of his birth, in fact, is receding all the time further into the past, just as a leaf on a river flowing past us is all the time receding further into the distance. Would anyone wish to deny this ? And was ' time's flow ' ever intended to mean anything more ?

But is getting older to be called a ' change ' (however invariably it may be accompanied by changes) ? I do not see why not. To begin with it is the case that no more than ten years have passed since X was born, and then this is not the case ; to begin with it is not the case that eleven years have passed since X was born, and then this is the case. What was the case ceases to be the case, and what was not the case comes to be the case ; if this is not change, what is ?

But at what rate does this change, if it is one, occur ? How fast does one get older ? Surely the answer to this question is obvious. I am now exactly a year older than I was a year ago ; it has taken me exactly a year to become a year older ; and quite generally, the rate of this change is one time-unit per time-unit. Nor does any mysterious ' super-time ' enter into this calculation. It has taken exactly one year of ordinary time for my age to increase by exactly one year of ordinary time, and that is all there is to it.

But is this a ' rate of change ' at all ? How this question is answered is unimportant ; it is a question like ' Am I my own sibling ? ' If by ' sibling ' is meant ' other child of same parents ' I am not and cannot be my own sibling ; if what is meant is just ' child of same parents ', I am and must be my own sibling ; and I am and must be the child of my own parents whether I call this ' being my own sibling ' or not. If a ' rate ' of change is a ratio between something else and a time-interval, there is no rate at which my age changes ; if it is just a ratio between something (whether a different something or the same something) and a time-interval, my age changes at the rate stated above ; and in any case I become a year older every year (there are such things as birthdays), whether this be called a ' rate ' of a year per year or not.

A subtler difficulty has been hinted at by Pears, interpreting McTaggart. If X's birth ' is ' an event now ten years past, must it not somehow be present in order to have this present property, and must it not have been present a year ago to have at that time the different property of being nine years past ? Pears rightly rejects such suggestions, and shaves this new Platonic beard in the time-honoured way, scrapping those systematically misleading abstract nouns and replacing them by verbs. Discard ' X's birth is now ten years past ' for ' It is now ten years since X was born ', and how the pseudo-problems flee away ! It is clear now that what I am talking about is not really X's birth but X, and just as the pseudo-noun ' X's birth ' disappears, so does the pseudo-adjectival phrase that pseudo-qualifies it—' ten years past '—and instead we have an introductory phrase that is quite clearly not adjectival but adverbial in force.

This is exactly how I myself would solve the Pears-McTaggart problem ; I do not quarrel with the least bit of this solution (or dissolution). I would only not have it forgotten that adverbial introductions like ' It is now ten years since— ' can modify, beside verbs like that of ' I was born ', verbs like that of ' It was two years since— '. For ' It is now ten years since it was two years since I was born ' (' I had my second birthday ten years ago ') has nothing wrong with it whatever ; nor would longer chains of prefixes of this sort be in any way vicious (a man can perfectly easily commemorate not only his birth but one of his birthdays). This repetition is in order precisely because we have to do here not with a predicate, forming statements out of names, but with an auxiliary, forming statements out of statements, like ' not '. Add a predicate to the statement formed by adding a predicate to a name, and you get nonsense (like ' John runs runs '), but you can pile up ' not's until your arm gives in, and so it is with these others.

This means, too, that the ' is now ' in ' It is now ten years since— ' is not entirely nugatory ; it is a genuine present tense, for there might have been something else there instead (like ' was a year ago ') ; so that if one cares to reify one can say, not that X's being born, but that his having been born ten years ago, is now present, and one can also, if one pleases, call this an ' event '. Or, if one pleases, not. Having been born ten years ago is a logically parasitic kind of event (birthdays presuppose births and not *vice versa*), and one might want to reserve the term ' event ' for the labouring classes ; but (1) it is not a noxious parasite (what harm does it do ?—and what harm is done even if this flea also has smaller fleas upon its back to bite it, etc. ?) ; (2) all events, as Pears in effect shows, are logically parasitic in a measure (the problem ' Where is my birthday when it is past ? ' is no less teasing that ' Where is my birth when it is past ? ', but no more teasing either) ; (3) it is something that comes to pass in time (that I was born ten years ago comes to be the case and then to have been the case, exactly as my birth comes to be the case and then to

have been the case, and there is no more need with the one than with
the other to go out of time into a ' super-time ') ; and (4) not only
births, and not only birthday-parties, but also birthdays, are ' events '
in common parlance (a twenty-first is also a ' happy event ' to be
celebrated, and a different event from the celebration).

To put my point a little more polemically : Pears talks of truths
as the ' timeless shadows ' of events, and I don't think I believe in
these timeless shadows (such talk seems to me a strange inverted
Platonism—instead of time being the moving shadow of eternity,
eternity is the still shadow of time) ; but whether or not events have
timeless shadows, they do have shadows that are not in the least
timeless but follow them through time exactly as ordinary shadows
follow those who cast them through space.   These shadows, though
not the others, I think I understand ; and Plato, if I am not mistaken,
understood them quite well too.   Their law of projection is ' It is
m years since it was n years since p $=$ It is m $+$ n years since p ' ; and
this simple additiveness is the first premiss of the tortuous but not
at all mystical or mysterious reflections on age-comparison in the
Parmenides 154B-155D.

<div align="right">A. N. PRIOR</div>

Canterbury University College
Christchurch, New Zealand

THE LOGIC OF COGNITIVE STATES

JOHN STUART MILL reminded intolerant busy-bodies that ' to refuse a hearing to an opinion because they are sure that it is false is to assume that *their* certainty is the same thing as *absolute* certainty '. The reminder might seem to chime in nicely with Professor A. J. Ayer's discussion in *The Problem of Knowledge* of the topic 'Does knowledge consist in being a special state of mind ? ' But it might also suggest a faint similarity between Ayer's logic and that of the unwarrantably intolerant.

Such a suggestion would have considerable bearing on a number of issues where linguistic analysis has appeared to put metaphysics out of business. Let us examine a crucial stage in Ayer's argument : ' My point is that from the fact that someone is convinced that something is true, however firm his conviction may be, it never follows logically that it is true ' (chap. I, § iii). This reasoning is intended to establish the logical impossibility of cognitive states like self-guaranteeing intuitions and, presumably, a theological concept like knowledge by Faith, which may sometimes have to fly in the face of experience. Ayer applauds Professor J. L. Austin's article ' Other Minds ' for revealing the ' performative ' function of ' I know', an expression commonly used not to report a mental state but to vouch for a claim and lend it one's personal authority. Ayer continues : ' there would indeed be a contradiction involved in saying that he knew the statement to be true and that it was false ; but this . . . is because it enters into the meaning of the word " know " that one cannot know what is not true. It cannot validly be inferred from this linguistic fact that when someone is considering a statement which he knows to be true it is his state of mind that guarantees its truth.'

It is a favoured procedure of Ayer's to round on his opponents with the charge that they have obscured what is and what is not a logical necessity ; his *coup de grâce* is then to add that they are blindly demanding a logical impossibility. His claim to prove the absurdity of a cognitive state rests on the same foundations as do G. E. Moore's critique of ethical naturalism and as do modern versions of Kant's attack on the concept *ens necessarium*. In all three cases the argument takes the form : ' S is P ' cannot be a necessary truth because, by reference to current linguistic usage, we are not involved in contradiction if we ask ' Is S really P ? ' Any attempts to frame ' S is P ' in such new terms that it becomes a necessary truth are met with what might really be incompatible charges : (*a*) that we are cheating by smuggling the predicate into the newly formulated concept of the subject and (*b*) that we are dealing in logical absurdity. Both charges are, I suggest, illegitimate appeals to a Logical Absolute to save us the labour of examining relevant empirical evidence to see if experience does or does not

247

merit revolutionary concepts and a new entailment relation. A major inducement to such shirking is the way an investigation of meaning tends to become obeisance to a sacred cow. In our enthusiasm for analysis it is so easy to graft onto a descriptive sense of ' meaning ', like ' the way we do use and understand words ', a highly evaluative use with a force something like this : ' the way we should use words and would use them if our concepts were appropriate to reality '. For examples in the range of this latter use : a gentleman farmer purrs over his pint that he didn't know what ' peace and quiet ' meant until he gave up his job in the city ; a film star tells your roving reporter that she didn't know what marriage really meant until she met her fourth husband ; a Communist and a Catholic argue about what history, or truth or democracy happen to mean.

The word ' logical ' is closely affected by this particular ambiguity of ' meaning '. Thus, if ' S is P ' does not follow from the meaning of S in the former descriptive sense, it is tempting to slip from description to evaluation and deride the creation of a new ' S is P ' entailment by a non-commonsensical formulation as a logical absurdity, in a ' phoney ', absolute sense of ' Logical '. Perhaps the two people who feel the greatest need for such conceptual reformulations are the scientist and the ideological convert. Perhaps, too, the latter has some claim to a small share of the tolerance enjoyed by the former. For the cry of ' logical absurdity ' is too often a mere sacrosanctification of established usage and a return to the idea of logical absolutes, which analytical philosophy has itself done so much to discredit. It too often affirms *a priori* that new depths of experience cannot justify new ways of thought, whereby new entailment relations are created by making logically composite, interdependent concepts out of notions that were logically discrete. It also embodies the old myth that the world has the same logic as the language of the British upper classes.

' Appeal to prevalence is philosophically pointless.' Equally pointless is the attempt to promote a head-on collision between analysis and traditional metaphysics over the concept of knowing, by stressing one or two uses of a word with so many possible functions. ' I know ' may very often amount to no more than a personal authorisation or the brandishing of an Achievement Word. But we must dwell sympathetically, too, on examples from outside the range of hard-headed, ' liberal ' opinion. The ' I know ' of theologians and their faithful, or of the ' presciently ' gifted or of Old Testament prophets in modern Marxist dress can often involve a highly metaphysical affirmation about their state of mind. The analysis of an ' I know ' demands careful delineation, not reckless demolition.

In his attacks on cognitive states of mind Ayer seems to neglect what would be Wittgenstein's and his own good counsel by confusing states of mind with consciousness. His lack of patience with anything remotely mystical leads him to ignore the possibility that

intuition, like Faith, might call for a conceptual apparatus utterly removed from the elsewhere fruitful dichotomy of dispositions and occurrences. Marcel's *Metaphysical Journal*, for instance, hints at such an apparatus when he readjusts the concept of communication. But Ayer's lack of patience with existentialists leads him in this same section to lump them all together in a supporting role behind Sartre's account of consciousness as a *néant*. Such impatience indicates the likelihood of a destructive analysis missing the target, if the marksman lacks all sympathy with the view attacked. But what is most to be deplored in a champion of empiricism and analysis like Ayer is his misinterpretation of the word ' logical ', so as to assume, in Mill's words, that his certainty is the same thing as absolute certainty. Uncomfortably for us, the real problem of cognitive states must remain an empirical, not a logical problem. A truly rigorous empiricism might lead us back to some alarmingly old-fashioned answers.

<div align="right">JOHN KING-FARLOW</div>

Duke University

NOTE ON '⊃' AND '⊢' IN WHITEHEAD AND RUSSELL'S *PRINCIPIA MATHEMATICA*

WHITEHEAD and Russell's *Principia Mathematica* does not acknowledge or admittedly symbolize modal considerations in logic. Further than this, when C. I. Lewis' strict implication first came to his attention, Russell said, "whether or not there be such a relation as he speaks of, it is in any case one that mathematics does not need, and therefore one that, on general grounds of economy, ought not to be admitted into our apparatus of fundamental notions".[1] Nevertheless, the Frege sign (⊢) as used in *Principia* may be interpreted as a sign of logical necessity. The major relation in every formula covered by '⊢' holds necessarily. To take a specific case in *Principia*, the horseshoe (⊃) has different logical import when it is the major relation in an asserted formula than it has when it is a subsidiary or unasserted relation, and the Frege sign marks this difference of import regardless of whether or not attention is explicitly directed toward it. Thus, one modal consideration, logical necessity, is symbolically recognized (but obscurely) in *Principia*.

It is inaccurate to say that in the system of material implication a false proposition implies any proposition. Russell, on page 99 of vol. i of *Principia*, gives this as the interpretation of *2.21 ⊢ : ∼p . ⊃ . p ⊃ q. Several objections may be made to this interpretation, but the one especially relevant to the present *Note* is this : a proposition that is merely false without being necessarily false does not imply its own contradictory. Thus, the most that should be said is that a false proposition implies every proposition minus one—its own contradictory. A necessarily false proposition, however, implies its own contradictory. In this case, the usual statement holds : a necessarily false proposition implies every proposition.

To illustrate, let us take what are sometimes called the paradoxical theorems.

$$*2\cdot5 \quad \vdash : \sim (p \supset q) . \supset . \sim p \supset q$$
$$*2\cdot51 \quad \vdash : \sim (p \supset q) . \supset . p \supset \sim q$$
$$*2\cdot52 \quad \vdash : \sim (p \supset q) . \supset . \sim p \supset \sim q$$

The formula ∼(p ⊃ q) . ⊃ . p ⊃ q, which has the remaining combination as its consequent, will not hold, however, because the consequent is the contradictory of the antecedent, and the antecedent is not necessarily false. The same is true, of course, for the parallel cases using the principle of transposition.

$$*2\cdot521 \quad \vdash : \sim (p \supset q) . \supset . q \supset p$$
$$(2\cdot522) \quad \vdash : \sim (p \supset q) . \supset . q \supset \sim p$$
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all hold, but $\sim(p \supset q) . \supset . \sim q \supset \sim p$ fails for the same reason as the transposition of its consequent above fails. Although the two theorems in parentheses do not appear in *Principia*, they are easily proved ; (2.522) by taking *2.51, applying *2.03 and Syll. ; (2.523) by taking *2.5, applying *2.15 and Syll.

Let us take, however, the theorem *2·08, $\vdash : p \supset p$. Here the horseshoe is asserted. Thus, in the formula $\vdash : p \supset p . \supset . p \supset p$, all horseshoes may be asserted even though only the major one is ; that is, all are assertable and state necessary relations. When the antecedent is negated, it becomes necessarily false and implies its own contradictory : $\sim(p \supset p) . \supset . p \supset p$ holds and is assertable. Any antecedent that is necessarily false yields a column of F's on the truth table ; thus, the column under the major horseshoe of the formula will consist of all T's no matter what the column of the consequent may be. The similar principle is true for a consequent that is necessarily true : it yields a column of T's, and the column under the major horseshoe will consist of all T's no matter what the antecedent may be. The denial of a contingent truth will never yield its own contradictory, but the denial of a necessary truth will necessarily yield its own contradictory. Thus, the ' $\supset$ ' that is qualified by ' $\vdash$ ' in *Principia* fills a different place in the structure of the system than the one that is not : ' $\supset$ ' symbolizes either of two structural relationships according to whether it is or is not the major relation of an asserted proposition.

This principle is true not only of ' $\supset$ ' but of the other so-called logical constants as well. The formulas $\sim(p \vee \sim p) . \supset . p \vee \sim p$ and $p . \sim p . \supset . \sim(p . \sim p)$ hold and are assertable. In $\vdash : p \vee \sim p$ and in $\vdash : \sim(p . \sim p)$, the ' $\vee$ ' and the '·' respectively are asserted relations and have a different structural import than when they are unasserted.

The situation here is similar to that in Boolean algebra where the symbols ' $=$ ', ' $\neq$ ', ' $\subset$ ', etc., serve as predicates and the symbols ' $\cup$ ', ' $\cap$ ', ' $-$ ', etc., serve as operations (connectives) ; except that the distinction between predicate and connective is not recognized by the use of different symbols in the propositional calculus. The distinction is implicit in the structure nevertheless. Any relational symbol serves as a predicate when it is the major relation of an asserted proposition ; otherwise it serves as a connective. The use of the same symbol for both purposes has not confused the actual processes of logic, for punctuation keeps straight the distinction between the major relation and those that are subsidary. The use of the same symbol for both purposes has, however, tended to confuse the understanding of the place of the symbol ' $\supset$ ' in logical structure.

It is true that the predicate of Boolean algebra logically depends on quantification, while an asserted relation in the propositional calculus does not ; nevertheless, the structural difference between the asserted and the unasserted instances of ' $\supset$ ' is still present

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in the propositional calculus. The possible connection between assertion and quantification may be debated but it can not be uncritically assumed. Quantification makes a propositional function into a proposition, and this may indicate a connection between the two, but even if there is such a connection, the distinction between the asserted and the unasserted instances of ' $\supset$ ' still holds.

Thus, in *Principia*, ' $\supset$ ' serves sometimes as a predicate and sometimes as a connective. It serves as a predicate when it is the major relation of an asserted formula. When it is in a subsidiary position, it serves only as a connective : but if it *may* correctly be asserted even though it is in the subsidiary position, it *may* serve as a predicate, and *modus ponens* is based upon this consideration. By virtue of the fact that ' $\supset$ ' may become a predicate, the rule of inference may be applied. The logical intension of all logically necessary truths is null. If the logical intension of the antecedent of a formula is null, and if the major ' $\supset$ ' of the formula is asserted, the intension of the consequent is null too ; thus, the consequent is logically necessary and may be independently asserted. This is the rule *modus ponens*.

As the Frege sign is the sign of logical necessity in *Principia*, and as *Principia* is a two truth value system, the Frege sign is here the sign of truth value necessity. Since the publication of Wittgenstein's *Tractatus Logico-Philosophicus*, this has been called 'tautology '. Lewis points out that every *asserted* material implication in *Principia's* logic can be replaced by a strict implication, which is designed to express logical necessity.[1] That this can be done is due to the fact that the formula is a tautology and is logically necessary ; and the Frege sign points out that it is. Systems of notation that do not use the Frege sign must nevertheless tacitly recognize the difference between the asserted and the unasserted relation. The application of *modus ponens* depends on the recognition.

Ordinary English language obscures this situation. The symbol ' $\supset$ ' is often read ' implies ' whether it is asserted or unasserted, but ' implies ' is a verb and makes an assertion. Take the theorem $\vdash : p \supset q . \supset . \sim q \supset \sim p$, for example. Instead of using ' implies ' for every occurrence of the horseshoe, we would better interpret the structure of the theorem by the reading " p implying q implies *not-q* implying *not-p* ". To be meticulous in reading the symbol ' $\supset$ ' always by the words ' if——, then——' does not solve the problem of the verbal rendering, for ' if——, then——' also makes an assertion, and if the antecedent itself is a conditional, the antecedent is read as an assertion. To preserve the logical distinction between the asserted and the unasserted relations while using the words ' if——, then——', we would have to invent some such grammatical monstrosity as " If p if-then-ing q, then *not-q* if-then-ing *not-p* ". The invention would have to be even more monstrous if

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[1] Lewis and Langford, *Symbolic Logic* (New York, 1932): see the discussion following theorem 18.7 on pp. 165-166.

we were to read the disjunction in $\vdash : p \lor \sim p$ by a verb ; or to read the first negation in $\vdash : \sim(p . \sim p)$ by a verb and the second not by a verb, yet the first, as qualifying the conjunction, is asserted and is logically necessary, but the second is not.

When any of the symbols ' $\lor$ ' or '.' or ' $\sim$ ' appears as the major relation in a formula covered by the Frege sign, it is an assertion as much as is ' $\supset$ ' when so covered, and the grammatical fact that we have no verb for any of them is an accident of language completely irrelevant to logical structure. Logical structure is independent of any interpretation that may be given it in a natural language.

H. N. LEE

Tulane University

in the propositional calculus. The possible connection between
assertion and quantification may be debated but it can not be
uncritically assumed. Quantification makes a propositional function
into a proposition, and this may indicate a connection between the
two, but even if there is such a connection, the distinction between
the asserted and the unasserted instances of ' ⊃ ' still holds.

Thus, in *Principia*, ' ⊃ ' serves sometimes as a predicate and
sometimes as a connective. It serves as a predicate when it is the
major relation of an asserted formula. When it is in a subsidiary
position, it serves only as a connective : but if it *may* correctly be
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H. N. Lee

Tulane University

ATTRIBUTES vs. CLASSES IN *PRINCIPIA*

I

QUINE has shown [1] that the distinction between properties in extension (classes) and properties in intension (attributes or propositional functions, in the appropriate sense) breaks down in the system of *Principia* if the following rule of inference is adjoined as a principle of individuation of attributes :

(Q) If $\vdash \phi x \equiv \psi x$, then $\vdash \phi = \psi$.

Quine's demonstration runs essentially as follows :

(1) [*20.18] $\vdash [\hat{z}(\phi z)] = \hat{z}(\psi z)] \supset [F\{\hat{z}(\phi z)\} \equiv F\{\hat{z}(\psi z)\}]$.

Letting $F\{\hat{z}(\chi \hat{z})\} = _{Df} \hat{x} \epsilon \hat{z}(\chi \hat{z}) = \phi$, we have,

(2) $[(1)] \vdash [\hat{z}(\phi z) = \hat{z}(\psi z)] \supset [(\hat{x} \epsilon \hat{z}(\phi z) = \phi) \equiv (\hat{x} \epsilon \hat{z}(\psi z) = \phi)]$.

(3) [*20.3] $\vdash x \epsilon \hat{z}(\phi z) \equiv \phi x$.

(4) $[(3), (Q)] \vdash \hat{x} \epsilon \hat{z}(\phi z) = \phi$.

(5) $[(2), (4)] \vdash [\hat{z}(\phi z) = \hat{z}(\psi z)] \supset [\hat{x} \epsilon \hat{z}(\psi z) = \phi]$.

(6) $[(4), (5)] \vdash [\hat{z}(\phi z) = \hat{z}(\psi z)] \supset (\phi = \psi)$.

Thus the addition of (Q) to *Principia* leads to the conclusion that properties of common extension are identical, removing the warrant for distinguishing between classes and attributes in the system of *Principia*.[2]

II

In an effort to reconstitute the class *vs.* attribute distinction in the face of an acceptance of (Q), Smullyan has proposed [3] a two-point programme. His first proposal is :

(S1) To interpret the *Principia* symbol "ϕ!" to represent the extensional counterpart of the property ϕ, so that ϕ! is ϕ taken in extension : $\phi! = _{Df} \hat{x} \epsilon \hat{z}(\phi z)$.[4]

[1] " On Frege's Way Out ", MIND, lxiv (1955), 145-159. Only the Appendix, given on the last two pages of the paper, will be under discussion here.

[2] In the modified system of *Principia* discussed in the *Introduction* to the second edition (p. xxxix) *all* functions are extensional by explicit stipulation. In *this* system there is, of course, little point in a discussion of the question of attributes *vs.* classes.

[3] " A Note on an Argument of Quine ", MIND, lxv (1956), 255-258.

[4] Smullyan does not explicitly give this definition of " ϕ ", but takes it as undefined, subject to satisfaction of the two axioms; (A) $(\exists \psi)(\psi!x \equiv _x \phi x)$, and (B) $\phi!x \equiv _x \psi!x \supset \phi! = \psi!$ (this is misprinted in Smullyan's paper). However, the stated definition is the logical outcome of Smullyan's discussion, since (1) it adequately formalizes the intended meaning of " ϕ ! " as discussed by Smullyan, and (2) it satisfies the axioms (A) and (B) in virtue of *20·3 and *20·13, respectively. This explicit definition of " ϕ ! ", as based on ideas of Smullyan induces a fundamental change in the interpretation of the " ! " symbol in *Principia*, where this is undefined. For example, the axiom of reducibility, *12·1, now states *not* that every function (or predicate) has a first-order (formal) equivalent, but that every predicate has an extensional (formally equivalent) counterpart.

Smullyan's second point is to insist that an explicit notation be introduced in *Principia* for the *scope* of abstracts, analogous with the scope-notation for descriptions in *14.01 :

(S2) To re-adapt *20.01 as definition for class abstraction :

$$[\hat{x}(\phi x)]F\{\hat{x}(\phi x)\} = {}_{Df}(\exists\chi)[\phi x \equiv {}_x\chi \,!x \,\&\, F\{\chi \,!\}].{}^1$$

When these innovations are introduced, Quine's line of argument is upset. For (1) now becomes,

(7) $\vdash [\hat{z}(\phi z) = \hat{z}(\psi z)] \supset \{(\exists\chi)[\phi x \equiv {}_x\chi \,! \,\&\, F\{\chi \,!)] \equiv$
  $(\exists\chi)[\psi x \equiv {}_x\chi \,!x \,\&\, F\{\chi \,!\}]\}.$

Letting $F\{A\} = {}_{Df} A = \phi$, we have,

(8) $[(7)] \vdash [\hat{z}(\phi z) = \hat{z}(\psi z)] \supset \{(\exists\chi)[\phi x \equiv {}_x\chi \,!x \,\&\, \chi \,! = \phi] \equiv$
  $(\exists\chi)[\psi x \equiv {}_x\chi \,!x \,\&\, \chi \,! = \phi]\}.$

Since the formal equivalence of identical properties is an inevitable consequence of any acceptable definition of property equality,

(9) $\vdash(\exists\chi)[\phi x \equiv {}_x\chi \,!x \,\&\, \chi \,! = \phi] \equiv (\exists\chi)(\chi \,! = \phi).$

(10) $[*12.1,{}^2 (Q)] \vdash (\exists\chi)(\chi \,! = \phi).$

(11) $[(8), (9), (10)] \vdash[\hat{z}(\phi z) = \hat{z}(\psi z)] \supset (\exists\chi)[\psi x \equiv {}_x\chi \,!x \,\&\, \chi \,! = \phi].$
But we cannot infer $\phi = \psi$ from the right hand side of (11). Thus, adoption of Smullyan's proposals pulls the teeth of Quine's proof that co-extensive properties are identical.

III

However, even if we adopt S1 and S2 to resolve the particular point of difficulty adduced by Quine, other, related difficulties continue to stand in the way of maintaining in *Principia* the class *vs.* attribute distinction, in the face of acceptance of (Q). Three considerations are especially relevant here :

No. 1 : Acceptance of (Q) renders it impossible to distinguish between a property ϕ and its extensional counterpart $\phi\,!$. This is shown by the argument :

(12) $[*20.3] \vdash x \,\epsilon\, \hat{z}(\phi z) \equiv {}_x \phi x.$

(13) $[(12)], (Q)] \vdash \hat{x}\epsilon \,\hat{z}(\phi z) = \phi.$

(14) $[(13), (S1)] \vdash \phi \,! = \phi.$

No. 2 : Acceptance of (Q) renders it mandatory to *identify* (*qua* attributes) demonstrably co-extensive properties (*e.g.* self-identity and non-self-difference). For if $\vdash \hat{z}(\phi z) = \hat{z}(\psi z)$, then by *20.14, $\vdash \phi z \equiv_z \psi z$, and so, by (Q), $\vdash \phi = \psi$.

No. 3 : Acceptance of (Q) forces upon us awkward and unnatural consequences regarding the interpretation of *Principia* symbolism, if we wish at the same time to avoid the undesirable consequence,

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[1] In Smullyan's paper, there is a typographical error in that the last occurrence of the symbol " ! " is omitted.

[2] Here, and in various instances below, the starred numbers refer to propositions of *Principia*, not as they stand, but *as reinterpreted under Smullyan's proposals*.

$\vdash [\hat{z}(\phi z) = \hat{z}(\psi z)] \supset (\phi = \psi)$. For example, in view of the interpretation of "ϕ!" embodied in (S1), we should like to be able to assert :

$$\text{(A)} \quad \vdash (\phi x \equiv_x \psi x) \supset (\phi ! = \psi !),$$

i.e. that formally equivalent properties have the same extensional counterpart. But by *20.14, $\hat{z}(\phi z) = \hat{z}(\psi z)$ implies the antecedent of (A), and by No. 1 above, the consequent implies $\phi = \psi$.

IV

A resolution of these additional difficulties entailed by acceptance of (Q) calls for a more drastic revision of *Principia* than is embodied in Smullyan's proposals. What is required is a modification in the definition of class abstraction *20.01—and correspondingly (S2) —in the following way :

$$\text{(D)} \quad [\hat{x}(\phi x)]F\{\hat{x}(\phi x)\} = _{Df}(\exists \chi)[\phi ! x \equiv_x \chi ! x \ \& \ F\{\chi !\}].$$

It is clear that any consequences regarding the property ϕ that are derived from considerations regarding its class abstract as defined by (D) will relate to its extensional counterpart ϕ!. In particular, if *20.01 is replaced by (D), two important theorems of *20 now read as follows :

*20.14 $\vdash [\hat{z}(\phi z) = \hat{z}(\psi z)] \supset (\phi ! x \equiv_x \psi ! x)$
*20.3 $\vdash x \epsilon \hat{z}(\phi z) \equiv \phi ! x$.

Note that if (Q) is applied to the second of these we merely o btain (S1) ; and that from the first theorem and $\vdash \hat{z}(\phi z) = \hat{z}(\psi z)$, (Q) empowers us to infer no more than $\vdash \phi ! = \psi !$.

Thus, adopting (D) in place of (S2) immediately does away with all of the problems raised in section III. And it does so without itself entailing any difficulties for the interpretation of class abstraction in *Principia*. Given the interpretation of "ϕ!" embodied in (S1), (D) appears in consonance with the express intention of the authors of *Principia* (p. 188) that a function of a class abstract be always an extensional function of the property that is involved, since the definiens of (D) contains only the extensional counterpart of this property.

V

Of course, another and more straightforward way of avoiding difficulties for the class *vs.* attribute distinction in *Principia* induced by the addition of (Q), is simply to reject it. After all, the authors of *Principia* nowhere hint at subscription to (Q), and—as Smullyan has justly pointed out—acceptance of (Q) is indeed incompatible with positions held by them. Thus from the standpoint of the *Principia* the simplest way of keeping from trouble would be to reject (Q), or rather, to accept it only in the weaker form :

$$\text{(Q')} \quad \text{If } \vdash \phi x \equiv_x \psi x, \text{ then } \vdash \phi ! = \psi !.$$

(Q′) leads to no difficulties in interpretation (if we accept (S1)), and does not in any way interfere with the class *vs.* attribute distinction, either *via* Quine's argument or by the difficulties raised in section III.

VI

In entertaining the proposal of section V, we abandon (Q) in favour of some other, narrower principle of individuation for attributes (properties in intension). What is its nature ? It must surely be, in the classical manner :

$$\text{(I)} \quad [F\{\phi\} =_F F\{\psi\}] \equiv (\phi = \psi).$$

The properties ϕ and ψ are identical if, and only if they are intersubstitutable in all contexts (including intensional contexts).

It is assured by (I) that identical properties are formally equivalent. For if $\phi = \psi$, let us take F as defined by : $F\{\chi\} =_{Df} \chi x \equiv_x \phi x$. Then $(\phi x \equiv_x \phi x) \equiv (\psi x \equiv_x \phi x)$, and therefore $\phi x \equiv_x \psi x$. Moreover, (I) permits us to distinguish between attributes and classes, and specifically between ϕ and ϕ !.

In rejecting (Q) and accepting (Q′) we take a step that makes sense only relative to the thesis,

$$\text{(T)} \quad (\exists \chi)(\exists \psi)[\chi\,! = \psi\,! \,\&\, \chi \neq \psi],$$

that is, that several distinct attributes (properties in intension) can correspond to one and the same class (property in extension). By (T) there is some property χ that has the characteristic F defined : $F\{\chi\} =_{Df} (\exists \psi)[\chi\,! = \psi\,! \,\&\, \chi \neq \psi]$. By (T), then, $(\exists \chi)F\{\chi\}$. Now the extensional counterpart of χ, *viz.* χ!, either has the characteristic F or it does not. Assume, on the one hand, that $F\{\chi\,!\}$. Then, $(\exists \psi)[\chi\,! = \psi\,! \,\&\, \chi\,! \neq \psi]$, and therefore $(\exists \psi)(\psi\,! \neq \psi)$. On the other hand, if χ ! does not possess F, then $\chi\,! \neq \chi$ by (I), and indeed by any acceptable criterion of identity. Thus (I) does not compel us to identify ϕ and ϕ !—to the contrary, if we are willing to postulate (T) we are compelled by (I) *not* to make this identification.

To be sure, we do not by accepting (I) as definition of *identity* for attributes solve any of the really complex problems that can arise in this domain. But we do at least absolve ourselves of the charge that we do not even know what we *mean* by the term.

<div align="right">NICHOLAS RESCHER</div>

Lehigh University

17

THE LANGUAGE OF MOTIVES

IN chapter IV of his *Concept of Mind* Professor Ryle has argued that motives are ' propensities, not acts or states ', that they are traits of character or behaviour trends, not happenings or occurrences ; and that motive words are used ' to signify tendencies or propensities '. I shall try to show that while he is right to deny that motives are acts or states or feelings he is wrong to say they are propensities. He is wrong because, unless I am making an enormous howler, he is guilty of a ' category ' mistake. He has supposed that a motive is, to use a vague word, some kind of thing ; that motives are of the same general type as moods, agitations, habits, reflexes, traits, attitudes, though, of course, all these differ much among themselves. This mistake is not confined to Ryle. Philosophers generally, including those who criticise his treatment of motives, have perpetuated it.[1] It seems also to be common to modern textbooks of psychology, though their use of the term ' motive ' is too varied to make a sure judgement possible. I shall confine the illustrations of my criticisms to Ryle, but I wish to emphasise the generality, and hence the importance, of this mistake, if it is a mistake.

Motives, in my opinion, are not kinds of things which feature in our explanations of human conduct, they are kinds of explanation. To ask for the motive from which some action was done is to ask for the reason why it was done, although, as we shall see, only certain kinds of reasons are called motives. We can say either that the motive or the reason for his action was such and such, *e.g.* a love of justice, a desire for power, ambition, but we cannot say that the reason for his action was such and such a motive. Motive and reason are of the same logical category whereas motive and disposition—and, similarly, motive and occurrence—are of different logical categories. Motives are a class of reasons, they are not a class of dispositions. Being of a certain disposition or having a certain propensity may be the reason why a man does what he does, may explain his conduct, may furnish the motive of his action, but the motive is not itself a disposition or propensity. Even if, which I shall later try to show is not so, only lasting traits can be motives, motives are not lasting traits. To talk as if motives and traits and attitudes and habits were co-ordinate is a mistake for the same reasons as it is a mistake to talk in the same tone of the occurrence

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[1] *E.g.* McCracken and Peters in ' Motives and Causes ' *Proc. Arist. Soc.* Suppl. xxvi (1952) ; Toulmin, Flew, and Peters in ' Logic : Psychoanalysis and Morals ' in *Philosophy and Analysis* ed. Macdonald ; Barnes and Falk in ' Intention, Motive and Responsibility ', *Proc. Arist. Soc.* Suppl. xix (1945). Two recent exceptions are Toulmin in a postscript to his above mentioned article and, especially, Peters in ' Motives and Motivation ', *Philosophy*, vol. xxxi (1956).

of mental and of physical processes or of a display of gymnastics and team spirit.

Many things can be reasons, perhaps only a few can be motives, but reasons have not the logical characteristics of the things which can be reasons nor motives of the things which can be motives. Motives, like reasons, are imputed or denied, far-fetched or plausible, ostensible or ulterior, on the surface or underlying, apparent or real, main or subsidiary, involved or simple, reasonable or unreasonable, whereas the things which can be motives are long-term or short-term, occurrent or dispositional, physiological or social, due to this cause or that, aroused by this circumstance or the other. On the other hand, both motives, or reasons, and the things which can be motives, or reasons, are strong and weak, one and many, hidden and apparent, sinister or of the best. And, of course, to name the motive or give the reason is to name or give one of the things which can be motives or reasons. The question " What was his motive ? " is answered by mention of the things which can be motives, *e.g.* ambition, jealousy (traits), the overthrow of the dictator, money (the objective), the hope of avoiding an accident (?), a contempt for foreigners (attitude) etc. The fact that often we can say the same things about motives and about the things which can be motives, and that to name a motive is to name one of the things which can be motives is perhaps the main source of the confusion of motives with the things which can be motives, and shows why much of what Ryle, and others, say about motives is right for the wrong reasons.

A kindred reason for Ryle's mistake is his use of the word ' incli- nation ' as an alternative to ' motive '. An inclination is certainly a disposition, perhaps it is even a synonym of it, but I doubt that ' inclination ' is a synonym of ' motive '. To ask for the British Government's motives for the invasion of Egypt is not the same as to ask for their inclinations, though the latter may have furnished the former. The fact that a man is inclined to do something may provide a motive for his doing it but the motive is not the inclination itself. This mistaken identification of motive and inclination is of particular importance when Ryle is discussing the relation of ' motive ' to ' agitation '. One source of an agitation is " when an inclination is thwarted by the hard facts of the world ". This is perfectly true of inclinations but not of motives. Inclinations can, but motives cannot, be thwarted. Perhaps it is significant that in this paragraph (pp. 93-94) Ryle uniformly prefers the word ' inclination ' to the word ' motive '. The practice, common in some philosophers and psychologists, of using ' motive ' and ' desire ' as synonyms has the same faults.

The distinction, to which I am trying to call attention, between a motive, or reason, and the things which can be motives or reasons may be illustrated by the following rough analogy, though I do not say that the analogy by itself furnishes an argument. Members of Parlia- ment are not as such to be characterised as barristers or clergymen,

university lecturers or civil servants, over twenty-one or minors, although the people who are eligible for membership of Parliament can have some of these characteristics and not others. Members of Parliament as such are backbenchers or Ministers, in the Cabinet or not, members of the Government or of the Opposition, representatives of this or that constituency. To examine the characteristics of the people who can be members of Parliament is not to examine the characteristics of members of Parliament, although since members of Parliament are people they have also the characteristics of people.

Perhaps another way in which to make plausible the distinction I am emphasising is to recall two other fine-drawn distinctions which are better known. There is first the distinction between the correct view that we can and often do teach someone the meaning of a word by pointing to an object, and the incorrect view that the object pointed to is the meaning of the word. The second is the distinction between the correct views that we normally say ' I know that p ' if and only if we are confident that p, and that we can express our confidence by saying ' I know that p ', and the incorrect view that ' I know that p ' and ' I am confident that p ' are sometimes synonymous.

People behave as they do behave usually because they are as they are. Hence the most important clues to the reasons or motives for any action are to be found in the character of the actor. Ryle is therefore justified in looking for motives among traits of character though not in identifying motives with traits. Because people have the characters and the life histories that they have and because they are in the situations they are in, they are furnished with, provided with, certain motives. But when they are being accused of having such and such a motive they are not merely being accused of having the character or the history that they have, or of being in the situation they are in. Sometimes people act out of character but are not unmotivated. A sudden desire or fear or the momentary noticing of something may be our motive for an action. We are usually unable to discover the motives for the actions of a stranger because we do not know him well enough ; we sometimes mistake the motives of a friend because we know him too well.

There is no logical connexion between the view that some dispositions are common to all men, some to particular societies, and some to a few individuals and the view that some motives are universal, some general and some particular ; but there is a strong causal connexion between the two states of affairs.

Incidentally, a trait such as vanity, indolence, shyness, arrogance, love of justice, can, I think, be given as a reason for an action in two senses, only one of which is a motive type of reason. When there is no idea of a purpose or goal but only the exercise of a disposition, the disposition does not serve as a motive, though it is a reason. Vain people often do vain things almost automatically. Or they may act vainly in order to satisfy their vanity, in which case vanity

provides the kind of reason we call a motive. " ' No love ', quoth he, ' but vanity sets love a task like that '." The psycho-analysts' talk of unconscious motives tends somewhat to blur this distinction because, if I understand them, they seem to hold that all exercises of a character disposition are due to a need to satisfy that disposition. Similarly, while the having of a disposition can furnish a motive for an action, it might on the other hand compel a man to do something. But when we are compelled, the notion of a motive does not come in.

Since the dictionary defines motive in a very flexible way, namely, " that which ' moves ' or induces a person to act in a certain way ; a desire, fear or other emotion, or a consideration of reason, which influences or tends to influence a person's volition ; also often applied to a contemplated result or object the desire of which tends to influence volition ", there are many sorts of reasons for an action which are called the motives of the action. To use the word " motive " of the reasons why people behave as most of us normally, naturally and usually do is, perhaps, to be influenced by the psychologist's technical terminology. Ordinarily, I suspect, we look for a motive when the action is unusual to a person or abnormal in our group. We speak of a conscious motive when someone knows the reason for his acting as he does. Whenever we do anything carefully and deliberately because we think it will have such and such results, then we can say that the wish to obtain these results was our motive. So we ask what were his (or your) reasons and not merely what were the reasons. We speak of an unconscious motive either when we are unaware of any reasons why we did what we did or when the reasons we think we had do not furnish as plausible an explanation of our actions as other reasons of which we are unaware. Provided in this case we are talking of the sort of thing which could, in another case, serve as our (your, his) reasons. There is nothing strange about being unconscious of our motives in these cases.

As has been generally recognised, the main feature which distinguishes motives from other kinds of reasons we offer to explain conduct is that of an objective or aim or purpose or goal. That is why so many motives can be stated in terms of ' a desire for . . . ' ' a want of . . . ' ' a wish to . . . ' and their opposites. These terms, incidentally, seem usable to describe either the occurrent and conscious anticipation of a goal or the dispositional, and often unconscious, tendency to act as if seeking a goal. If a person is the passive victim of, is compelled by, something whether it be ' external ' such as a letter or a blow or a threat or a promise or whether it be ' internal ' such as a fit or a mood, that is, something which possesses him, then his actions have reasons but not motives. Again, if he acts automatically or habitually or aimlessly, then his actions have reasons but not motives. To mention the situation or circumstances in which he finds himself is also to suggest a reason but not to provide a motive.

The reason for an action is called a motive either if the action is done with a purpose or if it is the sort of action for which a purpose or objective can be plausibly suggested.   In the former case we call the motive conscious, in the latter unconscious.   It is this feature of purposiveness [1] which distinguishes those reasons we call motives from other sorts of reasons and which makes us refuse the title to compulsive, automatic, habitual or aimless behaviour.   And this is in fact how Ryle distinguishes those dispositions he calls motives from those dispositions which are moods, agitations and habits. " An inclination is a certain sort of proneness or readiness to do certain sorts of things on purpose."   " Nor do we enquire with what object an embarrassed man blushes, stammers, squirms or fidgets."   " The sense in which a person is thinking what he is doing when his action is to be classed not as automatic but as done from a motive, is that he is acting more or less carefully, critically, consistently, and purposefully."   The antithesis of acting from a motive and not acting from a motive is bound up with the antithesis of purposive and not purposive, and not with that of dispositional and not dispositional.

Ryle's assimilation of motives to dispositions leads him to falsify the relation of motives to moods (pp. 99-100).   He makes these differ only in that moods are monopolising and characteristic of the total set of a person during a short term, whereas the opposite is said to be true of motives.   But this is the difference between moods and traits.   Similarly it has led critics to accuse him of failing to distinguish attitudes from motives when his failure is really to distinguish attitudes from traits.

Again, the question as to how ' motive ' is related to ' intention ' seems to me the question whether an explanation of a person's conduct in terms of intentions can be said to be a motive type explanation, and not the question whether a motive is or is not part of, or an element in, an intention, whether it precedes or follows it, etc. (e.g. Barnes and Falk, op. cit.).

Finally, it must be mentioned that Ryle frequently does, as I understand him, regard motives as kinds of reasons and not as kinds of things which can be reasons.   He says, for instance, that " the imputation of a motive for a particular action is not a causal inference to an unwitnessed event but the subsumption of an episode proposition under a law-like proposition " (90).   Indeed, it has been said (Peters, " Motives and Causes," op.cit. p. 146) that what, according to me, he occasionally and rightly does is what he always and wrongly does.   He is accused of using ' motive ' as a blanket term.   That is just what it is.   In so far as Ryle does use motive in what I have argued is the correct way, the only question which

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[1] Besides ' purposiveness ', Peters in ' Motives and Motivation ' stresses ' context ' and ' conclusiveness ' ;   cf. Nowell-Smith, *Ethics*, chapts. 8 and 9. I am not sure that these are *distinguishing* characteristics of the kinds of reasons called motives.

arises is whether he has unduly narrowed its application by confining the things which can be motives to dispositions.   I have incidentally levelled this charge against him.

<div align="right">A. R. WHITE</div>

University of Hull

NOTES

THE PERROTT STUDENTSHIP IN PSYCHICAL RESEARCH

The electors to the Perrott Studentship invite applications from candidates. The Studentship, which is tenable for one year, will be of such value, not exceeding £300, as the electors may determine.

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JOINT SESSION
Southampton University, 11-13 July 1958

PROGRAMME

Friday
11 July.   8 *p.m.*   *Chairman:* Professor R. I. Aaron.
            Address by Mr. A. M. MacIver, " Knowledge ".

Saturday.
12 July.   10 *a.m.*   (a) *Symposium:* The Autonomy of Ethics.
              Chairman : Professor D. Cousin.
            Mr. R. Holland, Professor H. D. Lewis.

            (b) *Symposium :* Complementarity.
          Chairman : Professor S. Toulmin.
            Mr. P. Feyerabend, Dr. D. M. MacKay.

            8 *p.m.*   *Symposium :* Nonformal Entailment.
              Chairman : Professor G. Ryle.
          Mr. C. Lewy ; Mr. Watling ; Mr. P. Geach.

Sunday.
13 July.   10 *a.m.*   (a) *Symposium :* Good at . . .
              Chairman : Professor R. C. Cross.
            Mr. C. K. Grant, Mr. A. White.

            (b) *Symposium :* Authority.
          Chairman : Professor H. B. Acton.
Mr. R. S. Peters, Mr. P. G. Winch, Professor A. Duncan-Jones.

            8 *p.m.*   *Symposium :* Pretending.
          Chairman : Mr. Stuart Hampshire.
Professor J. L. Austin, Miss G. E. M. Anscombe.

MINKUS-BENES ON INCORRIGIBILITY

MR. MINKUS-BENES' remarks on incorrigibility[1] reveal a position which is not wholly free of ambiguity. What I shall try to do is to state the alternative interpretations which present themselves, and show, then, that they all involve serious difficulties.

He first explicates the notion of incorrigibility in the following way : " In saying that the statement ' I feel a prick ' is incorrigible we merely . . . state that the sentence ' I feel a prick ' is under certain circumstances accepted as right and called ' true ' and under other circumstances rejected as wrong and called ' false ' " (p. 195). This statement, if taken literally, is false, and the position it states, therefore, is untenable. For statements like " God is watching over us " and " race x is superior to race y " satisfy the conditions here stated, and thus count as instances of incorrigible statements. Since incorrigible statements are *ipso facto* true, it follows that these examples are instances of true statements. It can obviously be shown, then, that there are many statements which become true on this account which we would not wish to call true. At least we would not wish to call them true merely on the grounds suggested, *viz.* that under certain circumstances they are accepted as right and called ' true ' and under other circumstances rejected as wrong and called ' false '.

Purportedly, this position is rendered more tenable by asserting that it is a fact of language that certain statements are incorrigible, or that the conditions stated above hold. But to say, as Minkus-Benes does, that a fact is an instance of a class of facts which we call facts of language if (but not only if) it can be expressed by the sentence " Certain sentences are under certain conditions accepted as right and called ' true ' and under other circumstances rejected as wrong and called ' false ' ", is to leave oneself open to the objections stated above. It is possible to find many counter examples. I do not wish to press this issue, however, since subsequent remarks by Minkus-Benes lead me to believe that he did not intend to offer this explication.

The next statement of the position is the following : " . . . there is, as a matter of actual fact about language, a use of the words ' I am feeling a prick ' such that if I now use these words in this sense or use, then I count my present feeling . . . alone as making certain, as settling conclusively, that the words should be asserted, rejected or be regarded as both true and false at a stretch " (p. 196). Let us examine this assertion.

Now, I am in sympathy with Minkus-Benes' claim that the objection to his thesis from those who say that no statements are incorrigible since it is possible that the expression one uses to make

[1] P. Minkus-Benes, " The Psychological Present ", MIND, lxvi, No. 262 (April 1957), 195-209.

264

the statement is incorrectly used is essentially beside the point. The interesting problem of certainty concerns the possibility or impossibility of errors of judgement, and not verbal errors. With this point conceded, I feel, however, that Minkus-Benes uses it in defence against an objection which has not been given a proper hearing.

Suppose, he says, after asserting " I feel a prick " I learn that I ought to have called the feeling in question a ' bump ' rather than a ' prick '. This is immediately assumed to be an instance of re-instruction in the language, and hence a case of mere verbal corrigibility, but I don't feel that this is the only feasible interpretation. There is no *a priori* reason to assume that the only way one can ever misdescribe one's experience is by either committing a verbal slip or by not understanding the expressions involved. Suppose I utter the sentence " I feel a prick " and later find out that I had been struck by a spherical object. Suppose, as well, I learn from a competent physician that my skin at the place where the object struck me is, and has been for some time, insensitive to things like pricks. The physician then sticks me with a needle and, to my surprise, I feel nothing. It is difficult to say whether or not I would continue to insist that I had felt a prick, but it would certainly be odd to say that, if I do take back my assertion, it is due to the fact that I have received re-instruction in the language. I might say that I probably made a verbal error, *i.e.* I really intended to say something else, but I might equally well assert that an error of judgement had taken place. If, indeed, the expression " I feel a prick " is to have a descriptive use, the logical possibility of mis-description must remain.

Of course, Minkus-Benes can accept what I have just said, and yet retain his thesis. For note that what he is actually asserting is an existence statement. He is saying that *there is* a use of " I feel a prick " such that the feeling which occurs at the time of utterance is sufficient to guarantee the truth, falsity or dubiety of the statement. If, then, evidence subsequent to the utterance *is* brought to bear on the statement, he can say that this case is not an instance of the use with which he is concerned. If, in the example mentioned above, I decide to say " Well, I guess I really misjudged the nature of the feeling I had by calling it a ' prick ' ", Minkus-Benes has the option of saying either that I must have made a verbal error or that my use of the expression " I feel a prick " is not an instance of the use which admits of no subsequent falsification. The first alternative, as I suggested, is rather arbitrary. That the latter alternative involves serious difficulties can be shown in the following way. If, whenever I decide to say that the statement " I feel a prick " is false, it is claimed that I did not use the expression in the incorrigible sense, it would seem that the only way of knowing whether or not I am using the expression in the incorrigible sense is to know whether or not it can be corrected. Or, if this is too strong, at least

one can say that in order to know whether or not my statement can be corrected, I have to know more than that there is a use of the incorrigible type. I have to know that I am, indeed, invoking that use. If the claim is that, although there is an incorrigible use, one does not know, in any particular case, whether or not one is using the expression in this way, the sceptic can remain as sceptical as he likes for he still has no guarantee that he will not have to surrender any particular assertion.

If, again, we do not take the statement of the position literally, but rather assume that Minkus-Benes wishes to supplement his existence claim with the claim that we can know that a certain use is of the incorrigible kind, I think that the burden of proof rests with him. The few remarks he does offer as evidence are not very convincing. He says : " . . . don't you remember what it is you do to find whether I truly say ' You feel a prick, don't you ? ', you remember that in such a case you just consider what you feel and hold this to the point in such a strong way that it would never enter your mind to wait for, wonder over, consult the future. You surely remember that you consider the words unconditionally acceptable provided only you feel a prick, for what else is there that you should remember you go by ? " (p. 197). One might construe the last sentence of this quote as an expression of the necessary truth that if I know that I feel a prick, then I know that the statement expressed by the sentence " I feel a prick " is true, but I do not think that the author intended to say this ; there is an appeal to the facts, *viz.* what is felt. The difficulty with this model is that it fits a lot of expressions which Minkus-Benes would not want to call incorrigible. For example, he says that " This is a table " is not incorrigible. And yet, when someone asks me whether or not a certain object is a table, I just consider what I see (or feel) and hold to this in such a strong way that it would never enter my mind to consult the future. I think that this is true for all of us. If, then, Minkus-Benes wishes to retain a distinction between the types of assertions which are corrigible and the types which are not, he will have to draw the line better than he has done. It seems to me, also, that the method by which the distinction is drawn should be less psychologistic than he suggests since the philosophical problem of certainty is not the problem of which statements will turn out to be falsified by contrary evidence, but rather, what class of statements, if any, remain immune to all possible evidence (in some sense or another of this expression ' possible evidence '). If the criteria which are chosen to distinguish the classes are grounded in the actual conditions of the world (as, for example, psychological criteria), many statements will turn out to be certain which ought not to turn out that way. Indeed, it is this very difficulty which upsets Minkus-Benes' efforts to draw the distinction via his model.

<div style="text-align: right">BERNARD BEROFSKY</div>

New York City

ON THE PARADOXES OF SELF-REFERENCE

I

NOT all self-reference is paradoxical, *e.g.* the first sentence in this paper is not. But some self-referring statements are paradoxical, *e.g.* the statement of The Liar. The question I want to discuss is whether we must rule *all* self-reference invalid, for the sake of banning the paradoxes. There is nothing to be said in favour of doing so, except that logic is a science, and its rulings are expected to possess a certain generality. We feel that " All self-reference is invalid " is much more logicianlike than the direct " The paradoxes are invalid ". For the paradoxes are a set of individuals, not a type ; and a statement which applies to a set of individuals merely is not a *principle*. Yet the object of the first edict is, and is no more than, to achieve the second. And there is a certain injustice in it, like punishing a class to catch one boy. Professor Popper has shown this very clearly in his socratic dialogue (MIND, April 1954). There is also a very good reason for not banning all self-reference ; namely the use which we make in philosophy itself of *reductio ad absurdum* arguments employing self-reference. It is surely legitimate enough to refute a too-critical philosophy by turning it onto itself—a point which could not be made if all self-reference were automatically invalid.

What, essentially, is wrong with the paradoxes ? The fact that they are paradoxical ! What, then, was wrong with the simple solution " The paradoxes are invalid " ? There was no generality in it. It does not say a certain type of statement is invalid ; it says merely that The Liar, Grelling's paradox, etc., are invalid. The answer therefore is to investigate The Liar, Grelling's paradox, etc., find out what makes them paradoxical, and then generalize it. This means finding the basic form of these paradoxes and producing a whole class of new paradoxes, of which The Liar, Grelling's, etc., are special cases. When we have found this form, XYZ, we can simply pronounce, " The form XYZ is invalid "—thus banning a class of statements all of which are paradoxical. The problem, reduced to essentials, is simply to generalize the paradoxes.

By working on these lines we may be able to rebut the absurd charge that the paradoxes somehow *discredit* ordinary language. They are, of course, a form of language-breakdown, but so are contradictions, and no one ever supposed that the possibility of formulating contradictions in some way discredited ordinary language. Contradictions however are commonplace and there is nothing amusing in them. The same is not true of the paradoxes : the paradoxes are unique, unusual, astonishing—and it is this extreme uniqueness which seems to put them in a different category. I hope to show that once this uniqueness is removed, the problem of their exclusion disappears.

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II

Let us begin with The Liar in its simplest form, " I am lying ". Is The Liar speaking truthfully or falsely ? (The significance of what he says depends on this.) If he is speaking truthfully its significance is that he is lying, *i.e.* that he is speaking falsely. Whereas if he is speaking falsely, its significance is that he is not-lying, *i.e.* that he is speaking truthfully. Thus each interpretation leads to the other ; but it does not stop there, for that leads back to the first, which in turn leads back . . . and so on. This shows that The Liar, whatever it is, is not a contradiction. It does not make two incompatible assertions ; it oscillates between two incompatible assertions and fails to settle on either one of them.

Now the paradox could not occur at all if it were not for the fact that a statement is capable of re-interpretation. The significance of any statement depends on whether the person making it is speaking truthfully or lying. If he is speaking truthfully we read the statement as it stands. If he is lying we negate the main verb before reading. Facts which directly affect our interpretation of the utterance, such as whether he is lying or telling the truth, may be called *pre-conditions* of the statement. Now if a statement refers to its own preconditions, p_1 and p_2 say, it may force a reinterpretation on us. If it can be devised so that on p_1 it says p_2, and on p_2 it says p_1, we have the basic form of the paradox of The Liar. The only remaining condition is that p_1 and p_2 must be comprehensive, *i.e.* one of them must be the case, and mutually exclusive. Under these circumstances a paradox inevitably results.

In order to generalize this type of paradox it is necessary to devise three or more comprehensive, mutually exclusive pre-conditions. The effect of these pre-conditions must be such as to cause us to read the statement differently ; and if the reading resulting from each leads to the next in cyclic order, a paradox will result. This may be achieved as follows :

Once upon a time there was a man called Arthur who suffered from the peculiarity of *selective prolixity* in speech. That is to say, his sentences contained redundant words which were not intended and which should be omitted in reading them. His speech-behaviour was studied by a team of psychologists and they came to the following conclusions : When he was *angry* the 3rd, 4th, 5th, 7th, and 8th words were redundant. When he was *drunk* the 3rd, 4th, 5th, 6th and 7th words were redundant. When he was *sober* the 3rd, 5th, 6th, 7th and 8th words were redundant. These patterns remained constant and applied to everything he said.

Since he was always either angry, drunk or sober everything he said had to be, so to speak, de-coded. And in order to do this it was necessary to know what condition he was in. Unfortunately this depended on Arthur's co-operation. On one celebrated occasion he would offer only the cryptic information,

" I am either angry, or drunk, or sober ".

If Arthur was angry, it read as " I am drunk ". If he was drunk, it read as " I am sober ". While if he was sober, it read as " I am angry ". But since it was known that he must be either angry, drunk or sober the statement was paradoxical. It was decided that Arthur's condition had taken a turn for the worse and he was removed to an institution where he could be properly looked after.

This form of construction could obviously enough be extended to produce paradoxes depending on 4, 5, 6, . . . etc. pre-conditions. There are an infinity of possible paradoxes of this type, which may be called *pre-conditional*, and The Liar is only one of them. The weakness of these paradoxes lies in the nature of the pre-conditions. Arthur was, as a matter of fact, either angry, drunk or sober ; and The Liar was, as a matter of fact, either speaking truthfully or falsely. In the case of Arthur's statement this enables us to draw the conclusion that he was lying—a conclusion which does not make his statement any the less paradoxical. Nor does it ' solve ' the paradox of The Liar to say that The Liar made a meaningless utterance.

III

It is well known that paradoxes essentially the same as The Liar can be produced in a variety of ways. The simplest of these is the form, " This statement is false ". And here, it is plain, the paradox is not a result of any reference to pre-conditions. A statement of the form " p is false " causes us to re-interpret p, and if ' p ' is the statement itself the possibility of a paradox occurs. To generalize this form of The Liar we require epithets applying to statements which will cause us to re-interpret them, but in a more selective fashion than ' true ' and ' false '. Since these epithets do not exist we have to define them ourselves. A statement will be said to be a $3/1$-*worder* if it is true when the first three words are read together with any one of the remaining words, providing that there is only one such selection that is true. For example, " Macmillan is a man, lion or tiger " is a $3/1$-worder, for " Macmillan is a man " is true, whilst replacing ' man ' by ' lion ', ' or ' or ' tiger ' gives a false or meaningless sentence. The following types of $3/1$-worders may be distinguished :

(1) A statement will be said to be *1236-TRUE* if it would be true if only the 1st, 2nd, 3rd and 6th words were read.

(2) A statement will be said to be *1238-TRUE* if it would be true if only the 1st, 2nd, 3rd and 8th words were read.

All statements which are not of types (1) and (2) will be called *123N^{68}-TRUE*. Thus every statement belongs to one and only one of the categories 1236-TRUE, 1238-TRUE and 123N^{68}-TRUE. Now consider the following statement :

" This statement is either 1236-TRUE,
    1238-TRUE or 123N^{68}-TRUE. "   (A)

(For the purposes of definitions (1) and (2) above, hyphenated words count as single words.)   It is clear that (A) is a 3/1-worder, for after " This statement is . . . " one and only one of the epithets ' 1236-TRUE ', ' 1238-TRUE ' and ' 123N⁶⁸-TRUE ' must make a true sentence (and the other possibilities ' either ' and ' or ' are meaningless).   Now if (A) is 1236-TRUE the 3/1 selection " This statement is 1238-TRUE " is true.   But if (A) is 1238-TRUE, the 3/1 selection " This statement is 123N⁶⁸-TRUE " is true.   In this case, since (A) is a 3/1-worder, it means that (A) is either 1234-TRUE or 1235-TRUE or 1237-TRUE.   But of these the first and the last are impossible, since the selections in question are meaningless ; therefore if (A) is 123N⁶⁸-TRUE the 3/1 selection " This statement is 1236-TRUE " is true . . . and so on.   It follows that (A) is a paradoxical statement.

This form of construction could obviously enough be extended to produce paradoxes depending on 4, 5, 6 . . . etc. epithets.   The epithets in question may be called *statement-epithets*, and the paradoxes *statement-epithet* paradoxes.   There are an infinity of possible paradoxes of this type of which " This statement is false " is only one example.   Paradoxes may also be produced using a variety of tricks of a similar kind, *e.g.* reading a sentence backwards or even inverting the letters of the words.

IV

Not all paradoxes belong to the two kinds already examined, *e.g.* Grelling's paradox about heterological does not.   In this paradox a new principle is involved, that of deriving a new sentence from the original by virtue of its *form*.   Thus " ' a ' is a " by virtue of its form implies that " ' a ' is homological ".   It follows immediately that if you replace ' a ' by ' not-homological ' a paradox will ensue. Grelling's paradox may be generalized as follows :

I call an epithet of the form " first x, then y, then z " a *triplet*. Triplets may be divided exhaustively into three kinds.

(1) If " first x, then y, then z " is x, but not y or z, we call the triplet *primo-dominant*.

(2) If " first x, then y, then z " is y, but not x or z, we call the triplet *secondo-dominant*.

(3) If " first x, then y, then z " is z, but not x or y, OR any other possibility except (1) and (2), we call the triplet *tertio-dominant*.

Our examples are similar to those used in the definition of homological.   For primo-dominance : " first long, then of moderate or average length, then of negligible or infinitesimal length " is long. For secondo-dominance : " first illegible, then mispelt, then orthografic " is misspelt.   For tertio-dominance : " first adverbial, then prepositional, then adjectival " is adjectival.   From the definition, it follows that no triplet can belong to more than one of the three categories.   By virtue of the additional clause in (3) the three

categories are comprehensive : thus every triplet is either primo-, secondo- or tertio-dominant. Now consider the question,

" Is " first secondo-dominant, then tertio-dominant, then primo-dominant " primo-, secondo- or tertio-dominant ? "

The question must admit of an answer ! Now if the answer is primo-dominant, by virtue of its form the triplet must be x, *i.e.* secondo-dominant. But if the answer is secondo-dominant, by virtue of its form the triplet must be y, *i.e.* tertio-dominant. Finally, if the answer is tertio-dominant by virtue of its form the triplet must be z, *i.e.* primo-dominant OR neither of these. But we know that the answer must be one of the three, therefore it must be primo-dominant. It follows that the question is a paradoxical one.

This form of construction could obviously enough be extended to produce paradoxes depending on 4, 5, 6 . . . etc. formal implications. There are an infinity of possible paradoxes of this type, which may be called paradoxes of *formal* self-reference.

V

The paradoxes are not contradictions. They are not statements which point in two or more incompatible directions, but statements which oscillate between different directions and fail to point in any one steady direction. Just because of this behaviour they fail to work as statements ; it is their oscillating character which makes them invalid. The problem of the paradoxes disappears, for they are excluded by the edict, " Oscillating statements are invalid ". This requires no more justification than the edict, " Contradictions are invalid ". Like contradictions, oscillations do not always appear at once. A set of definitions may entail a contradiction, without this being immediately evident ; so also it may entail an oscillation, without this being immediately evident.

It is true of course that the examples of oscillating statements given in this paper are artificial ones ; or at least more noticeably artificial than the original paradoxes of The Liar and Grelling's. But so are most of the examples given in any textbook of logic of, say, syllogism. It is not the artificiality of the examples that is important but the fact that they exist. The fact that there is an infinity of possible oscillating statements of at least three different kinds (I do not claim that these are the only kinds) shows that their extraordinary uniqueness—as it appeared formerly—was an illusion, and that to ban them it is not necessary to banish also the entire multitude of innocuous self-references, direct and indirect, which occasionally appear in every field of discourse.

C. P. WORMELL

Brighton College

IX.—NEW BOOKS

Facts and Faith. By REGINALD O. KAPP. Riddell Memorial Lectures. University of Durham. Oxford University Press, 1955. Pp. 63. 5s.

SOME books deserve to fall stillborn from the press. A collection of confusions more comprehensive than Professor Kapp's lectures could not be discovered—indeed could hardly be invented. His argument is unintelligible, so it cannot be summarized. A word about his intentions must suffice, followed by short discussions of the author's treatment of (1) Monism-Dualism, (2) Randomness-Orderedness-Diathetes, and (3) Physical Laws.

Kapp's question is "whether reality . . . consists only of the material universe, as is claimed by those philosophers who are called 'monists', or whether it . . . comprises [also] . . . non-material influences, as is claimed by those philosophers who are called 'dualists' (p. 4). He ultimately decides for dualism.

(1) *Monism-Dualism*. Kapp construes these as answers to a genuine problem, the only possible answers. The genuineness of a philosophical inquiry into the ultimate constituents of reality is never questioned. Nor does Kapp discuss why 'monism' and 'dualism' are the only possible conclusions for such an inquiry. Typical monistic doctrines are ". . . mechanism, emergent vitalism, holism, logical positivism, cybernetics. The supporters of each have claimed . . . that at last the problem was near solution . . ." (p. 7). "According to monism, matter constitutes the whole of reality" (p. 9). Hegel and Bradley are dismissed in a footnote as irrelevant to Kapp's discussion of monism. "The monist claims that all ordered as well as all random events occur as a result of the unaided action of matter on matter" (p. 18). ". . . according to monism order results from *what matter does*; according to dualism order results from *what is done to matter* . . ." (p.12). "If I were satisfied that the laws of physics do make for order [!] I should support monism" (p. 24).

The purpose of Kapp's book, therefore, is to learn "How To Decide Between Monism And Dualism" (p. 16). Monism is an inadequate empirical hypothesis; it cannot account for the existence of order in the universe. But dualism remedies this deficiency; it supplies the causal ingredient which makes for the existence of order. That Kapp does construe this as an empirical issue is clear from page 15 : "If acceptance of monism . . . would cause our scientific textbooks to be re-written so would acceptance of dualism."

What is this ingredient ? The author's 'irrefutable' conclusion (p. 56) is that order is explicable only if there exist non-material causes. Kapp designates these with the 'non-evocative' word *diathete*. Examples of diathetes are the mind, the soul, entelechy, *élan vitale* (p. 9). "Dualism is the belief that the whole of reality is composed of two parts, one named matter and the other diathetes."

(2) *Randomness-Orderedness-Diathetes*. So another form of Kapp's question is "whether non-material influences (diathetes) have a real existence or not . . ." (p. 8). The question, however, is always changing its shape. Besides being 'influences', which 'sometimes act on matter' (pp. 9, 10, 12), diathetes are 'active realities' (p. 12), 'things' (p. 13); they 'lack location' (p. 12), are 'necessarily nowhere' (p. 14), can 'exercise direct control over any living substance' (p. 17), are 'the source

of order ' (p. 20), and are the ' guidance, selection, control, order . . . introduced into a [physical] . . . system ' (p. 43) ; the energy of individual molecules can be ' subjected ' to them (p. 43), they can be intense (p. 44). " . . . diathetes is but another name for order " (p. 44).

Kapp is embarrassed by this richness. His ' problem ' now takes the form, " That diathetes, be they called God, life, mind, or the soul, can be nowhere and yet do things is a most puzzling concept " (p. 53). Furthermore, " Some things display a property that receives the name ' order ' " (p. 18), so diathesis is a property of things. But not all things. Some things lack diathesis, and " they display chaos " (p. 18).

" Events that lead to an ordered result are called ordered events and those that do not are called random events. The monist claims that all ordered as well as random events occur as a result of the unaided action of matter on matter " (p. 18). Kapp the dualist, however, disputes this claim. Hence he must account for ' order ' in another way, and diathetes are the chosen *explicans*.

The author treats *ordered* and *random* as antithetical. But his rich, beautiful prose confuses him again. *Random* occasionally means *empirical*. Thus in the physical world " anything may happen that is logically possible " (p. 36). From which Kapp concludes that such a world is ' random '—it is a chaos into which the physicist must introduce [!] ' system '. From the non-necessary character of physical statements he infers that " . . . there are no [physical] laws " (p. 52). " . . . the laws of physics do not prove that there is order in the inorganic world . . ." (p. 63). One is inclined to ask ' If they do not what could ? ' Kapp confuses a logical with an empirical issue here. He cannot see how a statement whose negation expresses a possible state of affairs can be a statement about anything other than random events. That is, only what is necessary can be non-random, ordered. This is a howler, nothing else. It is logically possible that the earth might move in a non-elliptical orbit. This does not entail that Kepler's first law concerns random events, and must be rewritten in statistical notation. Even here Kapp thinks otherwise. Writing of eclipses (' exceptional ' events (p. 47)), which he *fears* some will construe as ordered events and not random (p. 46), he concludes " Indeed it can be shown that if there is no principle to prevent the three bodies from following a random movement, they must follow the observed orbits " (p. 47). If this makes any sense at all, it must be this—that since paths other than those actually described by the eclipsing bodies are possible, therefore all possible paths are random ; hence the paths the bodies actually do follow are random, and, (somehow) that they could not follow any other paths. (*Cf.* also pp. 31, 32.) What one is supposed to conclude from this about the status of Newton's celestial mechanics, I am unable to fathom.

By this very ' argument ' however, Kapp vitiates his own conclusion : " The laws on statute books prove that there is an order in men's conduct. The laws of biology prove that there is order throughout the organic world " (p. 63, *cf.* also p. 41). Are not the negations of these laws logically possible ? Or is it analytic that motor cars keep to the left, and that acorns grow into oaks ? If not analytic, then in the legal and the biological world too " anything may happen that is logically possible ", *i.e.* what does happen is purely random—there are no statute book or biological laws either ; *reductio ad absurdum.*

All hope of understanding Kapp's interpretation of *randomness* must be abandoned on page 50 : " If an event can only be predicted with the

18

help of knowledge of the positions and motions of the particles concerned and of the forces brought to bear on those particles the event is a random one." On almost any standard account of randomness this is madness. But the end is not yet in sight. Another meaning of *random* arises when Kapp contrasts it with *planned*. Thus the stimuli which reach the brain " . . . are not co-ordinated. They may arrive from anywhere [?] They are random events, unselected, most of them the result of pure chance " (p. 20). In this antithesis, order must be what is planned for. So naturally in the ' rough, untouched world of lifeless things ' there is no order, no planning. For who ever planned the planetary orbits and the sequences of the eclipses ? Theologians may have an answer, but the cold logic of Kapp the scientist forces the unwelcome verdict that these natural phenomena are purely random.

Order fares no better. After considerable effort I own that I can make nothing of Kapp's position.

(3) *Physical Laws.* Again, elementary confusions : " A law that is conceptually true can, of course, nevertheless be verified by experiment. One can prove the multiplication table by counting oranges " (p. 60). And he argues on page 33 from the fact that in the ' rough, untouched world of lifeless things ' (a phrase which enchants him) the laws discovered in the physics laboratory are never exactly ' obeyed ', to the conclusion that there are *no* laws operative in that world, which is therefore ' random '. (*Cf.* pp. 45 and 52.)

The laws of physics are, for Kapp, just tautologies (p. 62). They are " conceptually, not empirically true " (p. 60). There are no exceptions to them (p. 58) ; if there were they would not be laws ! They are " true by definition " (p. 33), " . . . are the consequences of the way we think about things " (p. 32)—and yet they are somehow ' abstracted ' from, *e.g.* " all the properties of an electrical circuit " (p. 61). But they do not (alas) ' make for order ' (p. 24)—whatever that now means.

Indubitably, sentences and formulae are sometimes used in physics to express statements against which evidence is not possible, or at least inconceivable. Kapp is correct about that. But there are usually other uses for those same sentences and formulae which result in expressions against which evidence certainly is both possible and conceivable. This is the case with the many laws which experimental physicists characterize as ' merely empirical ', and must have obtained with, *e.g.* Boyle's Law as it was understood in 1661, and with Ampère's as it was appreciated in 1820. And Kapp should remember, or learn, that Leibniz and Poleni attacked Newtons' Second Law not for being self-contradictory, but for being (as they thought) empirically false. There must be some sense, moreover, in which Newton's *Principia* fails as an *empirical* hypothesis to account for the perturbations of Mercury ; [1] else how could its success in explaining the observationally comparable perturbations of Uranus be regarded as the consummate triumph of planetary mechanics, as it was by every practising astronomer between 1845 and 1905 ? History will not allow us to construe the discovery of Neptune as a proof of the empirical truth of celestial mechanics, but then to speak of restricting the scope of the theory when (by an exactly similar argument) Leverrier invented the ' planet ' Vulcan to account for the misbehaviour of Mercury. The restricted scope talk is illuminating. It calls attention to a feature of physical laws which is underestimated, or misunderstood. But there must be a sense in which

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[1] This despite the pronouncement of the *Tractatus* 6. 342.

nineteenth-century electro-magnetic theory failed empirically if we are ever to understand fully the impact on physical thinking of, *e.g.* black-body radiation, the photoelectric and the Compton effects, and electron diffraction. No justifiable account of all physical laws being tautologies pure and simple has ever been given. It could not be given. Surely Kapp does not give it. As a Professor of Electrical Engineering, Kapp shows a remarkable resistance to appreciating obvious features in the discoveries of Faraday, Maxwell, Hertz and Thomson, to mention just a few physicists who have reported ' merely empirical ' discoveries in their own work.

Even Kapp's title hides a confusion which permeates the book, the confusion between the faith of the religious person, *i.e.* his faith *in* God, and the faith of the scientist, *e.g.* his faith *that* " Like effects in nature are produced by like causes " (Newton, *Principia*, Book III). The author just stirs it all together as faith, pure and simple, and then marks the similarities between the scientist and the religious believer. It all makes one rather apprehensive about the next Riddell Memorial Lecturer. Will he be selected at random or on faith ?

On page 45 " . . . of the second law of dynamics . . ." should read " . . . of the second law of thermo-dynamics . . .".

NORWOOD R. HANSON

An Introduction to Deductive Logic. By HUGUES LEBLANC. New York, Wiley (London : Chapman & Hall, 1955). Pp. xii + 244. £1 18s.

THIS is probably the most thoroughgoing introduction to the propositional and predicate calculi at present available. What distinguishes it most noticeably from the average logic text of this kind is the way in which the significance of the more advanced stages of logic is briefly indicated at the appropriate places in the less advanced stages. There is, after all, no better moment to introduce, at least in outline, many-valued and modal logic, than immediately after explaining two-valued truth-functional logic. This, Mr. Leblanc does in ten very useful pages at the end of chapter one. Nor is it sufficiently often the case that an introductory text will stress the importance of the extended concept of validity which results from (and is frequently overlooked by the student in) extending the propositional into the predicate calculus. Yet Mr. Leblanc follows up his first presentation of examples of quantificational deduction with a section (at the end of chapter two) in which the significance of the proof that if a monadic schema is 2^k-valid then it is valid, is made fully evident. The proof itself, to which he refers in a footnote, is given in the last chapter along with outline proofs of the other main metatheorems of his two calculi. Again, in the exposition of the propositional and predicate calculi (chapter three) two versions are used. Version I is of the usual kind found in elementary texts, namely, the kind in which instances of axioms and theorems require separate proof. Version II enlarges on this by the use of parallel metaxioms and allowing for the inclusion of the rule of Substitution as a derived rule. This method of introducing the fully formalised logical calculi should be very helpful for the student who so often finds difficulty in, for example, the early stages of Quine (*Mathematical Logic*), having been accustomed only to the elementary Version I type

presentation. A neat and brief account of Gentzen's natural deduction technique concludes this chapter on formalised truth-functional and quantificational logic.

The logic of identity, classes and relations receives an outline formalisation in chapter four. Calling for particular mention here are the section (31) on numerical quantifiers (a topic rarely included in elementary texts) and its counterpart (36) on n-membered classes, and the sections (35 and 38) on the Boolean algebra of classes and of relations. These simplified accounts of the intertranslatability of Boolean class-formulae with monadic quantificational formulae and of Boolean relational formulae with formulae of the identity calculus, make for that much-needed sense of perspective which students find difficult to acquire. The whole of this chapter, which takes us as far as functions, should prove valuable preliminary material for courses using Quine (*Mathematical Logic*).

The final chapter (Sample Syntax) provides proofs of the metatheorems of consistency, completeness, decidability and independence for the propositional calculus and of decidability for the monadic predicate calculus. In addition there is a very useful outline of Gödel's proof that a formula is a theorem of the predicate calculus if and only if it is quantificationally valid. It is particularly useful in that the theoretical point of Skolem normal forms is brought out by their actual use in a proof. So many logic texts introduce the student to Skolem normal forms and then leave him in the air without a hint of any use to which they can be put. Also deserving to be favourably mentioned are the exercises on each chapter and the bibliography.

The demerits of the book are few and unquestionably outweighed by the merits. Among the former that might be mentioned are : (1) a very unilluminating introductory section of seven pages which discusses " dimensions of semiotic " and advocates a somewhat dubious classification in cumbrous terminology of the various branches of logic ; (2) a rather high-handed treatment of the philosophical issues that inevitably raise their heads from time to time (on page 64, for example, we are allowed to remain neutral on the ontological issue between Platonism and Nominalism without being told that, and why, neutrality is possible only as long as we do not advance beyond the restricted predicate calculus) ; (3) an exceptionally long and even then far from comprehensive accompanying list of errata.

G. B. KEENE

Aristotle's Metaphysics, edited and translated by JOHN WARRINGTON, Everyman's Library, 1956. Pp. xxvii. + 388. 8/6

IT is a disgrace that this extraordinary translation should have been published. It is marred by numerous and monumental blunders and not redeemed by any compensating clarity or felicity of style. The translator follows to a quite remarkable extent the Oxford translation by Sir David Ross (and the analyses in Ross's edition) ; and of course where he does this things run smoothly. But when he lifts his eyes away from Ross's version the result is often disastrous. I will list a few specimen aberrations as concisely as possible.

1013a14 : by τέχναι ἀρχιτεκτονικαί Aristotle of course means arts which govern and direct subordinate arts. Everyman's translation ' the

plastic arts and those concerned with architecture ' (p. 3) makes nonsense of the passage. 1014a26-31 : by absurdly supposing that the φωνῆς στοιχεῖα are *syllables* the translator makes nonsense of the passage (p. 7). 983b10 : τοῖς παθεσι μεταβαλλούσης does *not* mean ' undergoing modi- fication in its passive state ' (p. 57). 989a30-33 : Aristotle is speaking of a view which he thinks to be implied by what Anaxagoras said. Anaxagoras did not state this view explicitly, ἠκολούθησε μέντ' ἂν ἐξ ἀνάγκης τοῖς ἐπάγουσιν αὐτόν. He would have had to accept it if it had been put to him. Everyman, page 72 : ' he must have inherited it from his pre- decessors ' ! 991b12 : ' notwithstanding that ' (p. 78) completely mis- understands the (simple) Greek. 1028b1-2 : Aristotle has said that full knowledge of a thing is knowing *what it is*, not just knowing what it is like, how big it is, etc. He reinforces this by pointing out that knowledge of a quality or quantity is knowing what *it is*. The question τί ἐστι; can be asked about a quality as well as about a thing ; and it is the man who can answer this question who has real knowledge of the quality or the thing respectively : ἐπεὶ καὶ αὐτῶν τούτων τότε ἕκαστον ἴσμεν ὅταν τί ἐστι τὸ ποσὸν ἢ τὸ ποιὸν γνῶμεν. Everyman makes nonsense with ' we only know a thing's quantity or quality when we know what the thing itself is ' (p. 168). 1040b27-30 : Ross translates correctly : ' But those who say the Forms exist, in one respect are right, in giving the Forms separate existence, *if* they are substances ; but in another respect they are not right, because they say the one over many is a Form '. Everyman, page 203, leaves out an important bit and quite misrepresents Aristotle : ' If the Forms are substances, the Platonists are partly right and partly wrong in making them separate '. 1078b17 : Σωκράτους περὶ τὰς ἠθικὰς ἀρετὰς πραγματευομένου. Everyman, page 258 : ' Socrates, whose interest lay in character-building . . . ' ! 1079b6 : Everyman, page 260, takes over unchanged the translation and explanation given in the first edition of Ross's version. This is very retrograde ; for a convincing and important change of text, translation and interpretation has been incor- porated by Ross in the revised edition of his translation (1928) and in the corrected text he published in 1953 (although the commentary was un- fortunately not revised to suit the new reading).

Mistakes as egregious as these abound, together with some significant misprints (' odd ' for ' even ', p. 65, n. 3 ; ' four ' for—presumably— ' smallest ', p. 71). The translation is disreputable and—because of its low price and attractive look—dangerous. It is earnestly to be hoped that, if this version cannot be withdrawn and corrected, a cheap edition of Ross's translation may be made available for the general reader and the Greekless student.

A word must be said about the lay-out of this volume. The editor has rearranged the books of the *Metaphysics* so as to produce a reasonably logical order. Δ is printed first as an introduction to terminology ; then A, B, Γ, E (preliminary studies) ; then Z, H, Θ (Substance, Potency and Actuality) ; next M and N (criticism of the theory of Ideas) ; and lastly I (Unity and kindred notions) and Λ (the prime mover). α and the second part of K are printed as appendices ; the rest of K is printed with the chapters of B, Γ and E to which it corresponds. This sort of arrangement, which follows the lines of Ross's discussion in his Introduction to this volume, is reasonable and helpful—though not too much easement must be expected from any rearrangement of a work so complex and untidy. Some smaller points. The editor has printed as footnotes (marked ' -A ') some evidently parenthetical or subordinate remarks ; this is a good idea.

The use of numbered or lettered sections to display the structure of arguments or lists is also helpful. Each chapter is given a title or heading and there is an index of chapter headings at the end ; this may serve some purpose, but does not do the job that even a select index of subject-matter or topics would do. As for the editor's notes, some are explanatory (closely following Ross), others are scholarly references of a kind not suited to an Everyman translation. Does Everyman want to know that a proverb Aristotle quotes is to be found in Leutsch and Schneidewin's *Paroemiographi*, vol. i ? Or that the genus next above horse and ass is named ' bushy-tailed creature ' in *Historia Animalium* 491a1 ? The reader who is interested in that sort of thing can surely look up Ross's edition for himself.

J. L. ACKRILL

The Nature of Philosophy. By DAYA KRISHNA. Prachi Prakashan, Calcutta. Pp. 223. 15s. 6d. ; $2.25 ; Rs. 10s.

PHILOSOPHERS of the analytic movement have often been concerned with other academic disciplines, and have used their tools to obtain a clarification of the concepts of history, mathematics, the natural sciences, and religion. But though there have been studies in the philosophy of history, the philosophy of mathematics, the philosophy of science and the philosophy of religion, there have been few full-dress examinations by philosophers of the activity in which they are themselves engaged, and the public have had to rely on stray *obiter dicta* and very occasional articles to discover what analytic philosophers are about. Mr. Daya Krishna has made a sustained attempt to remedy the deficiency and to work out a coherent philosophy of philosophy. His book has many defects. Every other page is disfigured with misprints. The reader is made to work unnecessarily and unreasonably hard : the style is usually difficult, and in the earlier chapters often obscure : there are too many sentences like (pp. 15-16) " The new evolutional emergents—meaning in the strictest sense a new organization, whether with the quantitative variation or not, yet in either case having a determinate novelty with a determinate new mode of behaviour—continuously make manifest the inherent possibilities of behaviour that lie in the nuclear notion of the thing ". And any reader with the normal prejudice in favour of austerity in academic work who starts with the preface is unlikely to read further. This will be a pity ; for in spite of blemishes in this, his first, book the author has something to say, and his manner of saying it improves steadily throughout the book. He has attempted to make explicit the aims of philosophers and to characterize what distinguishes a philosophical problem from a problem in the natural sciences or mathematics on the one hand and in religion or art on the other. The accounts that philosophers have actually given of their work are easily shown to be inadequate : the aims which in fact inform much of traditional philosophy are inadequate also, because incapable of fulfilment. Philosophy has long attempted to be the study of the Real, the Rational, and the Valuational, and the " . . . triune identity of the Real, the Rational, and the Valuational is the key to the nature of philosophical thought and its ultimate presuppositions . . . ". This has led philosophers persistently

to commit the Ionian fallacy of trying to describe a whole universe of discourse in terms which have meaning only if there are some things to which they cannot be properly applied, the synthetic-a-priori fallacy of trying to construe propositions as both logically necessary and empirically non-vacuous, and the fallacies in moral philosophy of naturalism and heteronomy. In this century philosophers have avoided these errors only to fall into another, that of construing philosophy as something other than philosophy, as logic, as natural science, as history, or as some other discipline. Nearly half the book is devoted to a detailed refutation of such mis-assimilations as exemplified in the work of particular modern thinkers and modern movements. In a perceptive and, for the subject, remarkably lucid exposition of existentialism, it is argued that the existentialists, for all that they have experienced far more acutely than other thinkers the tensions of philosophy, have made even less progress towards resolving them, and have advanced solutions which are patently inadequate and fail to satisfy the requirements even of their own tenets. It is " experienced tension " that characterises philosophical problems, but it is a tension of a peculiar kind, arising out of a conceptual confusion, and demanding for its resolution a conceptual analysis. Under this fashionable, but somewhat vague, description it is possible to fit in most recognisably philosophical works, and Krishna has no difficulty in showing that the essentially philosophical part—as opposed to the cosmological, biological, sociological or mathematical parts—of the thought of Plato, Aristotle, Descartes and Russell is an exercise in conceptual analysis with the object of attaining an analytic clarification of conceptual confusions. Unfortunately Krishna goes no further. He does not explain why conceptual upsets occur in some regions and not others ; why traditional philosophy was so much con-concerned with Essences, Being, Becoming, Substance and Causality, and modern with Space, Time, Meaning and Value. Nor does he indicate what are the criteria of success in analysis : philosophers find themselves subject to tensions and relieve them as best they may. Nor does he answer his own earlier criticism of the thesis that philosophy is nothing but analysis. Yet these are the difficulties to which a philosopher must address himself if he is to hold that the nature of philosophy is Conceptual Analysis and mean anything much by it. Plato could have accepted the name and the *solvitur ambulando* account of the method.

The author has read widely and quotes freely, not only from philosophers in the Western tradition but from Indian thinkers of whom we know so little. He is able to cite many parallels which show that philosophic experience is not confined to the West, and that speculative wonder and existentialist distress have been endured and felt by Vedic Seers and Urdu poets.

J. R. Lucas

Christian Theology and Natural Science. Some Questions on their Relations.
(Bampton Lectures, 1956). By E. L. Mascall. Longmans, Green and Co. Ltd., London, 1956. Pp. xxiv + 328. 25s.

Dr. Mascall has written a book on science and theology that is both readable and, despite the disclaimer of his subtitle, very comprehensive. My immediate reaction to it was that he gives us perhaps too much science and too little theology. A large proportion of the book consists of rather

detailed accounts of scientific views, with a good deal of quotation from
writings by scientists or about science, and this material seems of unequal
relevance to the central theological themes that are touched on.   The book
would have gained in directness by being shorter ; and I think it might
both have been shorter and yet have offered Dr. Mascall more scope for
the development of his own views if he had taken other people's views
more for granted.   But possibly this reduces to the futile complaint that
Dr. Mascall has not written another sort of book than the one that he set
out to write : his way of writing is presumably in accordance with a de-
liberate policy, and there can be no doubt that he does the job of exposition
and quotation extremely well.
    Dr. Mascall considers that " conflict " between science and theology is
misplaced. This is certainly true ; and in these days is so generally
agreed to be true that there is no need to elaborate on it, and Dr. Mascall
wastes no time in doing so : what he prefers to do is to suggest that there
is " a large domain of thought in which it is possible for theologians and
scientists to engage in intelligent, good-humoured and fruitful conversa-
tion " (p. xxi), that is, that so far from there being a conflict between
science and theology there is a possibility of mutual aid which has not yet
been widely realised.   He writes : " Many of the most important intel-
lectual issues at the present day lie in . . . borderland realms, and there
is little hope of solving them unless workers in each region are ready to
help and be helped by those in the other and not merely to shoot them
down " (p. xx). This reads well ; but the suggestion is not, I think, as
important as it may seem.   It is only some aspects of " science " that lend
themselves to such co-operative activity ; namely, the " philosophical "
aspects of science, or perhaps I should say, philosophisings about science ;
and Dr. Mascall's own chapters illustrate this : " cosmology ", " contin-
gency ", " indeterminacy ", " creation ", " body and soul "—these are not
so much words used by scientists as words used by people who philosophise
about science.   And such people are working in much the same line of
country as are the theologians for whom Dr. Mascall speaks—that is, when
the theologians are discussing the sorts of subject dealt with in this book.
Co-operation between the theologian and, say, the nuclear physicist or the
neurophysiologist, *when the latter are doing their own jobs*, is not so easy to
imagine.   Cosmologists, whether they be scientific cosmologists or theo-
logical cosmologists, can certainly discuss things on the same level ; and
this goes equally for people who concern themselves about " the body-
mind problem ", whether they base themselves on science or on theology :
but this is because, to put it over-strongly, science, and even theology,
hardly come into it—both parties are in fact philosophers, of a kind.   Dr.
Mascall's suggestion that scientists and theologians can and should learn
from each other, in fact, seems to promise something more exciting than
his development of it in the course of the book turns out in the end to have
delivered. ⌊The notion of " borderline intellectual issues " is undoubtedly an
important one, but I think it needs careful examination.   Dr. Mascall
provides us with no clear account of what exactly this expression may
mean.
    But, these general criticisms apart, it is proper to say that this book
clearly is written on the basis of a knowledge of, and reflection on, the
history and present state of the sciences of a width, and depth, that few,
if any, professional theologians in this country could equal.   The lucidity
and wit of Dr. Mascall's style constitute an additional ground for recom-
mendation of his book.

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FESTUGIÈRE'S *Epicure et ses Dieux* is by now a minor classic in Epicurean studies, and its appearance in English, nine years after its original publication, is welcome. This edition contains many alterations and additions made by Festugière, especially in the notes, and an index prepared by the translator. The translation is easy to read, and the book nicely produced.

Festugière's aims are very limited : after brief chapters on the state of religion in the Hellenistic period and Epicurus's life, he discusses Epicurus's views on friendship and the good life, and the Epicurean attitude to the gods. A final chapter contrasts Epicurus's religion with the ' astral religion ' which originated in the *Timaeus* and was later adopted by the Stoics. Though the Epicurean doctrines which are discussed are regularly, and illuminatingly, related to their historical setting, the formal arguments which, as Cicero (N.D. I, 16, 43, ff.) tells us, led Epicurus to his religious theories are not even stated. As a result, the reader is left with the impression that Epicurus's religion rested only on a naïve argument from ' clear vision ' (p. 58) ; in fact, it is doubtful whether *this* argument weighed much with Epicureans.

In general Festugière, no doubt deliberately, ignores the cosmological and epistemological side of Epicureanism, and, despite the warnings on p. 33, we are given the idea that Epicurus was anti-philosophical, almost anti-rational. This book will not encourage good philosophers to turn to Epicurus, and this is perhaps a pity.

E. J. LEMMON

John Locke and the Way of Ideas. By JOHN W. YOLTON. Oxford : Clarendon Press ; London : Cumberlege, 1956. Pp. x + 235. 30s.

THE aim of this book is to describe the part which Locke's *Essay,* particularly the challenging implication of resting all knowledge upon ideas, played in the discussions of his contemporaries. Such a study, if well done as in this instance, can bring into the open many novel and interesting facts. However, this involves much quoting in support of one and the same point from books that are outmoded, and therefore it tends to be a little repetitive and also in the form of a chronicle. But perhaps this is unavoidable for the purpose of proving the points which Dr. Yolton wishes to establish, the most important of which, I think, are the following :

(1) The referents of Book I of the *Essay* are no longer a mystery, *i.e.* we know who the writers are who championed the doctrine of innate knowledge in the seventeenth century, and those who argued against it

detailed accounts of scientific views, with a good deal of quotation from
writings by scientists or about science, and this material seems of unequal
relevance to the central theological themes that are touched on. The book
would have gained in directness by being shorter ; and I think it might
both have been shorter and yet have offered Dr. Mascall more scope for
the development of his own views if he had taken other people's views
more for granted. But possibly this reduces to the futile complaint that
Dr. Mascall has not written another sort of book than the one that he set
out to write : his way of writing is presumably in accordance with a de-
liberate policy, and there can be no doubt that he does the job of exposition
and quotation extremely well.

Dr. Mascall considers that " conflict " between science and theology is
misplaced. This is certainly true ; and in these days is so generally
agreed to be true that there is no need to elaborate on it, and Dr. Mascall
wastes no time in doing so : what he prefers to do is to suggest that there
is " a large domain of thought in which it is possible for theologians and
scientists to engage in intelligent, good-humoured and fruitful conversa-
tion " (p. xxi), that is, that so far from there being a conflict between
science and theology there is a possibility of mutual aid which has not yet
been widely realised. He writes : " Many of the most important intel-
lectual issues at the present day lie in . . . borderland realms, and there
is little hope of solving them unless workers in each region are ready to
help and be helped by those in the other and not merely to shoot them
down " (p. xx). This reads well ; but the suggestion is not, I think, as
important as it may seem. It is only some aspects of " science " that lend
themselves to such co-operative activity ; namely, the " philosophical "
aspects of science, or perhaps I should say, philosophisings about science ;
and Dr. Mascall's own chapters illustrate this : " cosmology ", " contin-
gency ", " indeterminacy ", " creation ", " body and soul "—these are not
so much words used by scientists as words used by people who philosophise
about science. And such people are working in much the same line of
country as are the theologians for whom Dr. Mascall speaks—that is, when
the theologians are discussing the sorts of subject dealt with in this book.
Co-operation between the theologian and, say, the nuclear physicist or the
neurophysiologist, *when the latter are doing their own jobs*, is not so easy to
imagine. Cosmologists, whether they be scientific cosmologists or theo-
logical cosmologists, can certainly discuss things on the same level ; and
this goes equally for people who concern themselves about " the body-
mind problem ", whether they base themselves on science or on theology :
but this is because, to put it over-strongly, science, and even theology,
hardly come into it—both parties are in fact philosophers, of a kind. Dr.
Mascall's suggestion that scientists and theologians can and should learn
from each other, in fact, seems to promise something more exciting than
his development of it in the course of the book turns out in the end to have
delivered. ¡The notion of " borderline intellectual issues " is undoubtedly an
important one, but I think it needs careful examination. Dr. Mascall
provides us with no clear account of what exactly this expression may
mean.

But, these general criticisms apart, it is proper to say that this book
clearly is written on the basis of a knowledge of, and reflection on, the
history and present state of the sciences of a width, and depth, that few,
if any, professional theologians in this country could equal. The lucidity
and wit of Dr. Mascall's style constitute an additional ground for recom-
mendation of his book.

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(1) The referents of Book I of the *Essay* are no longer a mystery, *i.e.* we know who the writers are who championed the doctrine of innate knowledge in the seventeenth century, and those who argued against it

previous to the publication of the *Essay*; hence Cassirer's 'construc-
tivist' theory is disposed of. (2) The whole controversy around Locke's
doctrine of substance shows how very novel this doctrine, even the non-
phenomenalistic aspect of it, is as compared with the Cartesian view;
and that his notorious suggestion that matter might be able to think is
a necessary implication rather than a stray-product of it. (3) The less
rigid of the traditionalists in religion, despite their often severe criticism
of Locke's epistemology, came to apply some of his doctrines in their
own works and even in a more perceptive manner than his alleged
followers.

From this and other evidence it appears that Locke's fear of being
plotted against by a group of men unconcerned with truth was unfounded.
And throughout one gains the impression that, though many of his op-
ponents are at best of secondary importance, their criticisms are usually
to the point; that therefore they do not deserve the disdain with which
he generally received them.

A new edition of Locke's *Essay* would certainly benefit from Yolton's
discussion and the evidence he has collected.

                                                    W. VON LEYDEN

The Revolution in Philosophy. By A. J. AYER, W. C. KNEALE, G. A.
    PAUL, D. F. PEARS, P. F. STRAWSON, G. J. WARNOCK, R. A.
    WOLLHEIM, with an introduction by GILBERT RYLE. Macmillan
    and Co., 1956. Pp. 126. 10s. 6d.

THIS book consists of a series of broadcast talks, designed to give listeners
some idea of the great changes in topics and methods of philosophising
which have taken place during the last sixty years. It contains one
lecture apiece on Bradley, Frege, Logical Atomism, Moore, the Vienna
Circle, and Wittgenstein, and two lectures on the most recent develop-
ments. The various contributors have done their jobs well, and if the
book is regarded as a series of lectures there is very little in it for a re-
viewer to complain of. One might particularly recommend an admirable
account of Moore by Mr. Paul, and one of the Vienna Circle given by
Professor Ayer with his usual sureness of aim.

Regarded as a whole, the book has certain weaknesses due to its con-
struction in separate pieces each of the same brief length. This makes it
harder for the reader to get a general impression of the course of events;
here, however, Professor Ryle's introductory essay is very helpful. Also,
one cannot help wishing that the authors could have had a little more
space in print than was possible on the air. For instance, Mr. Strawson
and Mr. Warnock give a good account in general terms of the aims and
procedures of up-to-date philosophers; yet I think the general reader
may still be left somewhat uncertain as to what is being done and why
it is worth doing, for want of a few well-developed examples for which
there is not enough room.

As it is, this is a useful little book, especially since general accounts of
the course of modern British philosophy are so hard to come by. And
in its freedom from obscurity, pretentiousness and passionate partisan-
ship it furnishes a good example of the philosophy whose origins it describes.

                                                    C. H. WHITELEY

Plato: Philebus and Epinomis. Translation and Introduction by A. E. TAYLOR. Nelson. 21s.

THIS book is the work of many hands. Taylor's translations of the *Philebus* and the *Epinomis* were found by Professor Raymond Klibansky in the Edinburgh University Library, among a bundle which also included translations of the *Sophistes* and the *Politicus,* and of Leibniz's *A Discourse of Metaphysics.* The present volume contains two of the translations of Platonic dialogues, together with Taylor's long Introduction to the *Philebus.* The works have been edited and annotated by Professor Klibansky, Professor Guido Calogero, and Mr. A. C. Lloyd, who has supplied an Introduction to the *Epinomis.* Professor Klibansky and Miss G. E. M. Anscombe will jointly edit a second volume which is to include Taylor's versions of the *Sophistes* and the *Politicus,* and his Introductions to these two works.

The Introduction to the *Philebus* repeats and amplifies the interpretation which is familiar to readers of *Plato: The Man and His Work.* Among a great deal that is merely repetitive of Taylor's earlier writings there are some philosophical and historical points which are developed more fully and adequately than elsewhere. There are valuable remarks on the connections between Plato's later ethical doctrines and the *Ethics* of Aristotle. The editors have supplied notes which refer to commentaries that have appeared since Taylor wrote what is here presented. The purely philosophical passages have worn less well than the historical interpretations. Taylor was always inclined to philosophise from a soapbox, and his declamatory strictures on certain unidentified "modern" philosophers are conceived and executed in a style that has lost favour since his time. But he never fails to be incisive and provocative, and some of his remarks on pleasure and on the mind-body problem retain an interest for philosophers that is not closely dependent on their context in Taylor's interpretation of Plato.

The translations are brisk and efficient, but they are marred by some archaic or inelegantly ponderous phrases which the editors might well have removed. The time has gone by when one of Plato's characters, or anybody else, can be allowed to exlaim "Egad!"; and to use words like "oblivescence" and "decrement" and "disgregation" is to push the *Philebus* further than ever away from the graceful fluency of the *Republic.* But there is no doubt that the book as a whole deserves the labour and skill that the editors and the publishers have expended upon it.

RENFORD BAMBROUGH

Faith, Reason and Existence. By JOHN HUTCHISON. New York : Oxford University Press Inc., 1956. Pp. xi + 306. $4.50.

THE most interesting division in contemporary philosophy of religion is between those who see their task as empirical and those who see it as normative. Is the philosopher merely to describe the conceptual structure of religious discourse or is he able also to prescribe standards of rigour and clarity to which religious utterances must attain if they are to be allowed to be meaningful ? If the latter conception of philosophy of religion is accepted, then the possibility of revising and re-interpreting religious

beliefs to make them philosophically defensible will arise. If the former
course is followed, the philosopher can merely describe what belief is and
every form of religious belief must simply be taken as it stands, whether
it is accepted or rejected. This links the division between philosophers
to the controversies between liberal and orthodox theologians, who are
also divided as to whether Christian belief must be taken as it stands or
can be revised and re-interpreted.

Professor Hutchison's book moves uneasily between these alternatives.
Sometimes he describes the philosophy of religion as " the evaluation or
testing of religious and theological statements ". Elsewhere he defines
its function as " the important ancillary task of exhibiting the rational
structure of theism ". His practice is as inconsistent as his precepts. At
the outset he seems prepared to take a bold revisionist line, allows that
religious utterance is characteristically mythical, and quotes with approval
Niebuhr's aphorism that a myth " tells many little lies in the interest of
one great truth ". The radical implications of this are never faced,
however, and presently a phrase like " the theistic hypothesis " is used as
though it presents no fundamental difficulty.

One reason for the inadequacy of Professor Hutchison's book is that he
does not seem to have made up his mind where he stands theologically.
At least this is left unclear in his book and so one can never be quite sure
what the religion is that is being analysed. A second, even more obvious
reason is that his is a book which starts a hundred hares and pursues none
of them to the death. There is not a single topic which is discussed at
sufficient length.

<div align="right">A. C. MacIntyre</div>

The Philosophy of the Church Fathers.   Volume I : Faith, Trinity, Incarna-
     tion.   By Harry Austryn Wolfson.   Cambridge (Mass.) : Harvard
     University Press, 1956.   London : Geoffrey Cumberlege. Pp.
     xxix + 635.   80s.

Professor Wolfson of Harvard announced in 1947 his programme of
publishing, in the order in which they might be finished, the several parts
of his opus majus, in which he would show how (1) the best in Greek
philosophy and in Hebrew speculative imagination was drawn together
coherently by Philo ; (2) patristic and medieval philosophy was in large
part a development of the best in Philo ; and (3) the tower of faith which
had thus been erected was shattered by Spinoza.   The title of the project
was Structure and Growth of Philosophic Systems from Plato to Spinoza.
Part II, Wolfson's two-volume Philo, published in 1947, was the only part
that had seen the light prior to the appearance of the present volume.

Here, in the first of two volumes constituting Part III, Wolfson recounts,
through an integrated series of topical analyses, not only the background of
the Fathers' theology in classical Greek philosophy, in the Old and New
Testaments, and in Hellenistic thought, especially Philo's, but also the
development of their thought, on each of a dozen topics, from Origen and
the Gnostics through Augustine.   He displays, for example, the relation
of each element of the Trinity to ante-Christian ideas : first, God the Father
vis-à-vis the God of the Old Testament and of portions of the Gospels ;
then, the eternally existing Son, or Logos, vis-à-vis the Old Testament,

rabbinic, and Philonic ideas of the eternally existing Messiah and of Wisdom or the Logos as the repository of the Platonic Archetypes ; and, thirdly, the Holy Spirit *vis-à-vis* the Old Testament and rabbinic holy presence.

Wolfson's command of the sources—Greek, Latin, Hebrew, Syriac, and other—seems greater than any person could acquire in one lifetime. It appears, for example, that he has organized into his own categories the infinite riches of all 300-odd volumes of both the Greek and Latin series of Migne's *Patrologia*. While he is occasionally more confident than seems warranted of the historical relation between one man's use of a concept (*e.g.*, faith) and that of a predecessor of one or more centuries earlier, in general his constructions are sound, and often they are unimpeachable. In the dominion of the learned, our author must be accorded royal status.

W. GERBER

Philosophical Papers and Letters. By G. W. LEIBNIZ. Translated and edited by LEROY E. LOEMKER. Chicago, 1956. Pp. 1228 + xvi. Price 90s.

THESE volumes, reproduced from typescript, contain the fullest selection of Leibniz's work yet available in English. The papers are arranged in chronological order and the selection has been well-made, being fully representative of all Leibniz's philosophical interests. Not only the familiar important works, the *Discourse*, the *New System* the *Monadology* and the *Correspondence with Clarke* are to be found here but also much that will be new to the English reader. One paper, *On the Elements of Natural Science*, has not been published previously. It is of considerable interest. The influence of Spinoza is clear from the language, but the treatment of the subject-matter, as Professor Loemker remarks, shows that of Bacon and Boyle, and in general the views of the arch-rationalist on empirical science are extremely sensible.

The reader who has only English at his command can obtain a clearer grasp of the scope and development of Leibniz's thought from these volumes than from any other source at present available. It is therefore all the more to be regretted that the work is marred by some inaccuracies in translation, especially from the Latin, which make it less than a completely reliable guide for such a reader. There are also occasional mistakes in the references forward from the Introduction to the Selections and odd slips such as the attribution to M. Daville in the bibliography of the interesting work " Leibniz Historienne ".

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1

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CONTENTS FOR VOLUME 8, No. 31, APRIL, 1958

CONTENTS

THE

Philosophical

Review

CONTENTS

JOURNAL OF THE HISTORY OF IDEAS

TABLE OF CONTENTS

ARTICLES

CONTENTS

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TABLE OF CONTENTS